The Royal Aeronautical Society

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with which is incorporated The Institution of Aeronautical Engineers

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PROCEEDINGS

FIFTH MEETING, FIRST HALF, 64TH SESSION

In the Chair: L. H. HOUNSFIELD, ESQ.

(President of the Institution of Automobile Engineers)

A Joint Meeting with the Institution of Automobile Engineers was held at the Royal Society of Arts, London, on Thursday, November 15th, 1928.

The CHAIRMAN (L. H. Hounsfield): Mr. Penn was technical officer at the Royal Aircraft Factory, Farnborough, where he was in charge of the experimental work on engines during flight and he was also proud to say that Mr. Penn was an old student of his.

Mr. PENN, before reading the paper, said that the work described, unlike laboratory work which is often the product of one person, was the product of many persons and had only been rendered possible by the able collaboration of both flying and technical officers at Farnborough. He would have liked to mention everybody who had taken part in it, but there was a long list of names. On the flying side the list commenced with Flight Lieutenant Bulman and ended at present with Flight Lieutenant Gray, whilst on the technical side there were technical officers such as Mr. Philpot, who had carried out much of this work both in the air and on the ground. Therefore, if any honours were due, they were due to the pilots and the technical staff at Farnborough who had been engaged in the air on the work. Finally, it must not be taken that the views expressed in the paper were necessarily those of the Air Ministry; indeed, they were purely his own expressions of opinion.

AERO ENGINE PROBLEMS IN FLIGHT WITH PARTICULAR REFERENCE TO CARBURATION*

BY

R. J. PENN, A.M.I.MECH.E.

Unlike the motor-car engine, which is generally designed, built and tested in association with the vehicle which it eventually propels, the aero engine is often designed and built with little or no knowledge of the types of aircraft in which it will be employed. The remainder of the power plant, fuel, water and oil systems of the car are also designed at the same time and to suit the chassis, but the aero engine, in the various types of aircraft, is likely to be coupled to many different forms of these systems, and consequently numerous

* Joint Meeting between the Royal Aeronautical Society and the Institution of Automobile Engineers.

problems arise between the completion of the acceptance tests of an engine and the final approval of an aircraft power plant for flight tests.

Again, the car power plant when once tuned up remains sensibly in tune, since the barometric and temperature changes on the ground are relatively small compared with those encountered by the aero engine. It is therefore the problems leading up to the final approval of an aircraft for flight, the methods of testing the additional parts of the power plant provided by the aircraft constructor, and the manner of circumventing the de-tuning that occurs the moment an aero engine leaves the ground, that are to be described.

The engine manufacturer having tuned and tested the engine under specified and controlled conditions to give the best performance under all running conditions, it is necessary to arrange the power plant and installation such that this performance is unimpaired.

As the efficient use of the power is not under review, it is not proposed to compare the efficiency of the transmission systems. It is sufficient to say, that the airscrew is designed to suit both engine and aircraft to give the optimum performance under certain defined operating conditions. Three main parts of the power plant have to be arranged for the engine and aircraft to operate successfully under all conditions of flight, but in the car two only, as the oil is generally carried in the sump of the engine.

Oil Systems

The evolutions of aircraft preclude the use of wet sumps; the approved oil for the engine, therefore, is stored in a separate tank, the capacity of which generally provides an air endurance of 50 per cent. in excess of that of the fuel. The transference of the engine to the aircraft has no effect upon consumption, the problems arise chiefly in cooling the oil in small aircraft of short flight duration, owing to the small heat reservoir. On the test bed it is easy to arrange that the accepted maximum inlet temperature of 70° C is not exceeded, but not so in flight. No attempt, however, is made to control the oil temperature definitely at 70°C., but to maintain it between the limits of 40°C. and 70°C. If the former were necessary, control of the cooling would be required when gliding with the engine throttled. The task of the aircraft constructor in providing adequate cooling is complicated by the variation in types of engines. They are found to vary in both ground-temperature rise across the engine and in the rates of circulation; the former between 10°C. and 60°C. and the latter between 35 and 180 gallons per hour according to the type of engine, and two extreme cases are known of circulation rates of 460-480 gallons per hour. Types of aircraft have been in use where only 4 gallons of oil are carried for a 400 h.p. engine; therefore, taking the medium rate of circulation 60 gallons per hour, the oil is circulated every four minutes or fifteen times per hour if the engine is flying at the normal bench revs. per minute.

The high rates of circulation have two effects; (*a*) the cooling problem is aggravated by bringing away more heat from the hot internal parts of the engine, through frequent contact with pistons, crankcase, etc., and (*b*) aeration of the oil becomes more pronounced, since the scavenge pump has a capacity ranging from 50 to 150 per cent. greater than that of the pressure-feed pump, and must frequently be returning air to the tank. For this reason a large air space is required in the oil tank. Fortunately the ground rise in oil temperature across the engine is not maintained in that most arduous form of flying, climbing, as the power of normally aspirated engines diminishes with increase in height, such that in 400 h.p. short duration aircraft, in which the initial rise in temperature is 40°C., coolers having an initial drop in temperature of about 7°C. have proved adequate. Further assistance arises in the temperature drop across the cooler increasing slightly with height. In supercharged aircraft however, the former does not apply, as the power and heat output are maintained

to a given height necessitating much larger coolers. In one case a battery of three oil coolers giving a drop in temperature of 30° was required as the oil was returning from the engine at over 120°C. Fig. 1 shows one of many types of cooler in use and Fig. 2 the flight test results of the cooling system of a 680 h.p. engined aircraft. The large temperature rise from 10° to 40°C. across the engine at 2,000 feet is accounted for by low rate of circulation through the inadequate flow of oil to the pump, and it is not until the whole

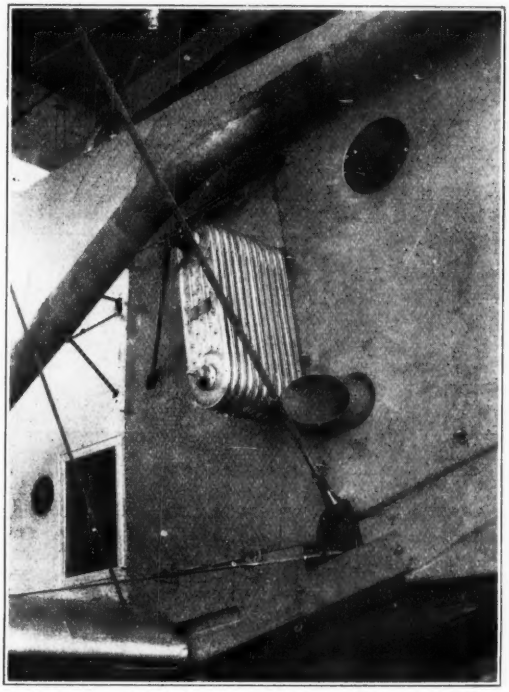


FIG. 1.

of the oil has been circulated at 7,000 feet and when the feed oil has reached a temperature of 35°C. that the normal flow from the tank occurs and the true temperature rise across the engine is recorded. A larger feed pipe, therefore, is necessary in this case for winter use. The slight increase in temperature drop across the cooler should be noted, while if the test had been conducted on a summer day the oil temperatures would have exceeded the maximum permissible, 70°C. More than eleven elements in the cooler therefore are necessary.

Unlike the water radiator no shutter or control is fitted, as the bulk of the oil is not carried in the cooler. When gliding with the engine throttled the rate of circulation is considerably diminished, and it is only necessary to arrange a relief valve in case the oil should become too viscous and tend to burst the cooler through rapid opening up. This relief valve is also of use when starting up on the ground in cold weather.

The problem of efficiently cooling the oil presents many difficulties and has been attempted in many ways. The commonest form of cooler is the container perforated with air tubes. The exposure of the underside of the oil tank to the slip stream, and perforation of the oil tank with air tubes have been tried with little success since they produce only 2 or 3° drop in temperature.

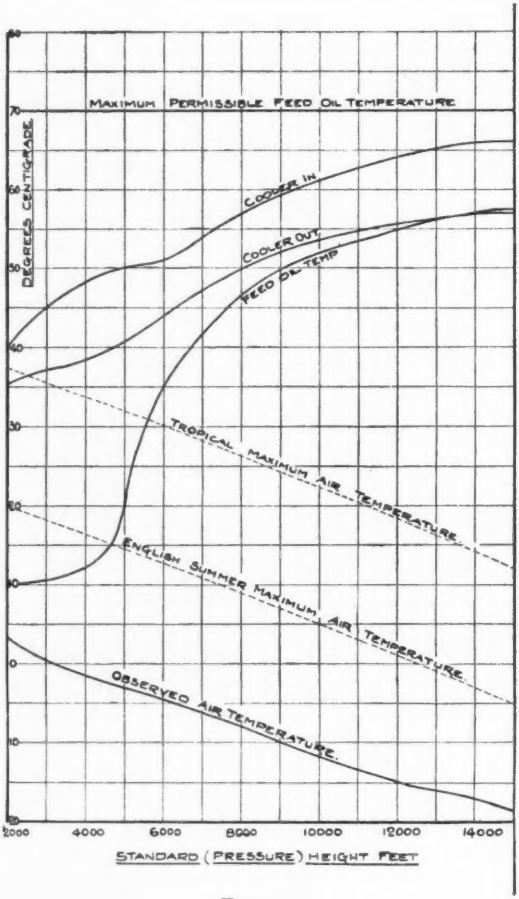


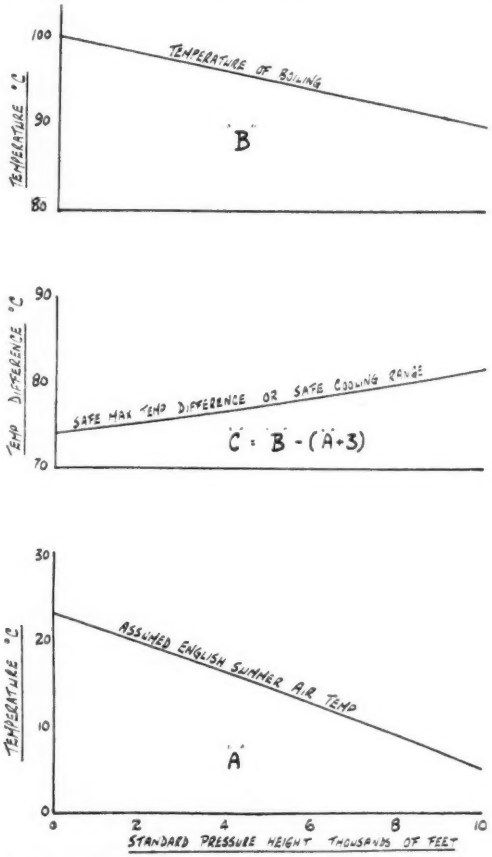
FIG. 2.

The type illustrated lends itself to increase or reduction of size quite readily as it is formed of thin elements. A turbine centrifuger, which not only cleans and de-aerates the oil but also cools it, has been successfully tried recently. Its further merits are that it is incorporated in the oil tank, requires no stays or supports, creates very little drag, and weighs very little. In view of the need for coolers providing a high drop in temperature due to the advent of superchargers, it appears that considerable use will be found for this cooler.

Water Systems

It may be said in a general sense that the radiator of a motor car is designed to meet the conditions obtaining when hill climbing in bottom gear on a summer day, and as there is seldom any means of controlling the temperature of the system, the radiators are often too large for winter use.

The aircraft radiator is designed for similar conditions, *i.e.*, climbing (low air speed) at full throttle on a summer day. The air temperature on the ground of a summer day is assumed to be +23°C. in England and +41°C. in the tropics.



Safe Maximum Temperature Range for Cooling. English Summer Conditions.

FIG. 3.

Whereas in many cars thermo-syphon circulation is provided, the circulation in aero engines is always by a pump which is designed to circulate at least 15 gallons per minute per 100 brake horse-power of engine output, against a circuit resistance of 2lbs. per sq. inch in excess of that of the engine with a pressure of 4lbs. per sq. inch below atmosphere at the inlet to the pump, whilst the water is maintained at 80°C.

The aircraft is affected in two ways, hardly realised in the motor car. With normally aspirated engines, during the climb both horse-power and air temperature decreases, but at the same time the efficiency of the radiator decreases slowly with decrease in air density. As all aircraft are fitted with radiator shutters or some equivalent device, if the radiator has sufficient cooling capacity for the climb, the pilot can regulate the water tempertaure for all other conditions of flight, observing the reduction in the boiling point with increase of altitude.

When an aicraft having a suitable radiator is climbing from the ground with water leaving the engine at, say, 75°C., the mean temperature of the system will gradually rise to a maximum and, at a certain height, commence to decrease. This is explained by the fact that though the radiator capacity tends to increase gradually over the engine requirements, its maximum cooling is only attainable as the water approaches the boiling point, and since the aircraft takes off well below the boiling point, the first part of the climb is made before the radiator has reached an efficient temperature, while it can store a certain quantity of heat.

In supercharged aircraft this point of balance naturally occurs at higher altitudes, usually at the height to which power is maintained. Considerably larger radiators are required to deal with the higher power where the indicated horse-power may be much greater than the brake horse-power. In one case of high altitude supercharging of a 450 h.p. engine 60 per cent. more cooling capacity than is usual with the normally aspirated engine was required, the increase in weight being approximately 70lbs., which in view of the considerably higher horse-power at altitude, is not disadvantageous.

It is obvious that the maximum temperature of the water at any height must lie between the temperature of boiling and the air temperature at that height. The difference in temperature between these two is known as the cooling range or maximum temperature-difference. It increases with height due to the lapse rate of the air temperature being greater than that of the boiling point.

Fig. 3 shows the assumed summer air temperature and the boiling points up to 10,000 feet. From the temperature-difference between curves A and B has been subtracted 3° as a factor of safety. C is the safe maximum temperature-difference or cooling range curve for England.

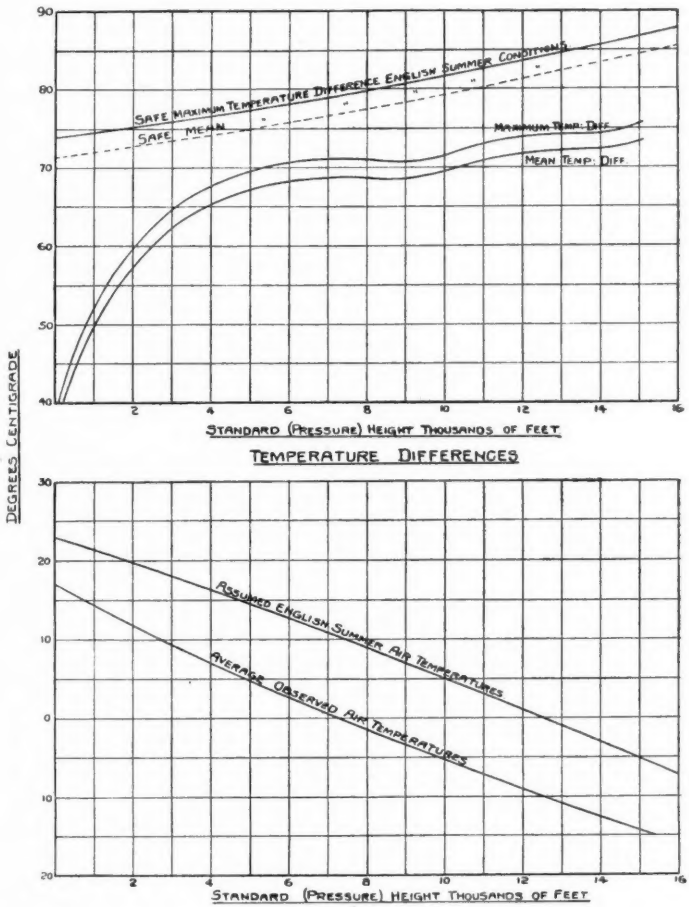
A similar chart can be prepared for tropical countries assuming 41°C. ground air temperature and the same lapse rate. The safe maximum temperature-difference or cooling range on the ground in this case would be $100°C. - (41° + 3°) = 56°C.$

The tests of suitability of the radiator are made by climbing the aircraft at full throttle and best air speed, and noting the following readings every 1,000 feet :—Temperature of atmosphere, temperature of water leaving engine, and temperature of water after passing through the radiator.

Fig. 4 gives the results of the flight tests of a radiator of 3.21 sq. ft. on a 680 h.p. engined aircraft. The small area of the radiator will be noted and is due to the use of long radiator tubes. The curves can be analysed as follows :—If the actual maximum temperature-difference curve rises above the safe maximum temperature-difference, then boiling of the water would occur on a summer day although it may not have occurred on the day of testing. In order to find the suitability of the radiator, the actual mean, and the safe mean temperature-difference curves are compared, and the suitability is measured at the height at which the actual mean temperature-difference curve lies tangentially nearest to the safe mean temperature-difference curve. The actual mean temperature-difference curve is obtained by deducting half the drop in temperature

across the radiator from the actual maximum temperature-difference, and the safe mean temperature-difference is obtained by deducting the same amount from the safe maximum temperature-difference.

At the height of 6.000 feet the safe mean = 75.7°C., the actual mean is 68.2°C. The suitability = Safe mean/Actual mean = 75.7/68.2 = 1.10 approx.


SAFE MAXIMUM TEMPERATURE DIFFERENCE ENGLISH SUMMER CONDITIONS
SAFE MEAN
MAXIMUM TEMP. DIFF.
MEAN TEMP. DIFF.
DEGREES CENTIGRADE
STANDARD (PRESSURE) HEIGHT THOUSANDS OF FEET
TEMPERATURE DIFFERENCES
ASSUMED ENGLISH SUMMER AIR TEMPERATURES
AVERAGE OBSERVED AIR TEMPERATURES
STANDARD (PRESSURE) HEIGHT THOUSANDS OF FEET

AIRCRAFT WEIGHT 8060 LB. AIRSCREW: WOODEN, WATTS 730.

FIG. 4.

The radiator of this particular aircraft is about 2 per cent. larger than is necessary as suitabilities of from 1.08 to 1.12 are generally insisted upon to meet various conditions.

The test just described is that applied to any new aircraft to determine the suitability of the radiator. The following data are always recorded for reference and comparison, and those given relate to the radiator of which the flight test figures have been quoted above.

Dimensions of radiator, $38\frac{1}{4}$in. × $13\frac{1}{4}$in.
Capacity of tube block, 3.225 galls.
Capacity of top tank of radiator, 1.0 galls.
Capacity of bottom tank of radiator, 1.375 galls.
Dimensions and type, 10 × 360 mm. R.T.6.
Radiator weight (empty), 146lbs.
Radiator weight (full), 202lbs.
Capacity of header tank to working level, 5.125 galls.
Airspace above working level, 1.25 galls.
Area of tube block, 3.21 sq. ft.
Capacity of tube block per sq. ft. frontal area = 1.005 galls.
Total capacity of system, 18 galls.
Flow through radiator with 7ft. head = 113 galls. per min.
Weight of shutters, 5.75lbs.

The flow test, *i.e.*, through the radiator with a 7ft. head, is important as it gives a measure of the hydraulic resistance of the radiator, and as it is known what the flow should be, it is possible to deduce whether during the construction of the radiator any restriction in the water-ways has occurred through defective workmanship.

It will be noted that no attempt has been made in the suitability tests to give quantitative results. This is due to the difficulty of devising a small and light water flowmeter, capable of recording up to 100 gallons per minute, suitable for flight test work.

There is another test which has been usefully applied to new aircraft, the vapour separation test. This is a test of the system to determine what quantity of water is lost when the water is inadvertently boiled. All systems are designed to have a reserve of water above the cylinders and this is carried in the header tank. Reserve water may be defined as the quantity carried in the cooling system in excess of that required to fill the engine, the circulating pipes and header, and to ensure the use of the whole of the radiator tube block.

The quantity of reserve water required is given by the formula :—

$$Q = 1 + \frac{PD}{1600}$$

where Q = Gallons of reserve water
P = Normal b.h.p. of the engine
D = Maximum duration (hours) of aircraft.

Fig. 5 shows an engine installation with two kinds of water systems (*a*) nosepiece radiator and header tank combined and (*b*) underslung radiator with separate header tank. The latter is often necessary for military reasons.

The amount of water lost during boiling is determined chiefly by the type of header tank, its water capacity and the amount of air space above the water.

The mode of procedure in the vapour separation test is as follows :— The engine is "run up" on the ground until the temperature of the water leaving the engine reaches 60°C. The revs. per minute are then increased to 75 per cent. of normal and allowed to remain constant up to the time of boiling and during the boiling period. All readings such as time, temperature and water loss, are measured from the moment this temperature has been reached. A large bore pipe approximately 15ft. long is connected to the vent of the header tank and led away towards the wing tip where vessels are kept in readiness to measure the water losses of the various portions of the test. The capacity of the system is measured when cold before the trial and the total

water loss is found next morning when the system is again cold by measuring the quantity of water necessary to bring the level up to the original height in the tank. The efficacy of the vent pipe is determined by the pressure generated in the header tank.

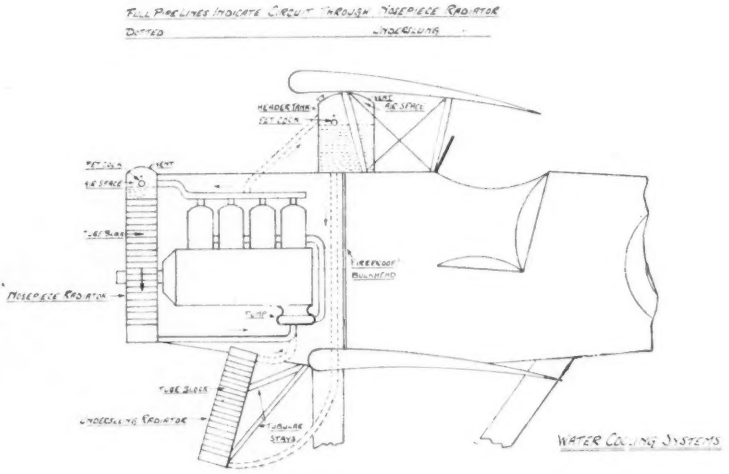


FIG. 5.

A certain aircraft employed on other engine experiments was found to lose water in flight. The results of the vapour separation tests are given in Table I.

TABLE I.

Vapour Separation Test.	Original Header Tank.	Experimenta Header Tank.
(*a*) Water in Cooling System before trial (water up to cock in header tank)	9.4 galls.	9.63 galls.
(*b*) Air space in header tank before trial	.75 galls.	1.47 galls.
(*c*) Air temperature during trial ...	14°C.	14°C.
(*d*) Time taken to boil	11 mins. 15 secs.	10 mins. 10 secs.
(*e*) R.P.M. during run up after water temperature reached 60°C.	1500	1500
(*f*) Water lost before boiling ...	2.03 galls.	0.75 galls.
(*g*) Temperature at which water loss began	89.5°C.	96°C.
(*h*) R.P.M. during boiling period ...	1500	1500
(*i*) Water lost during boiling period	.156 galls.	.05 galls.
(*j*) Total water loss, including item (*l*)	2.9 galls.	1.1 galls.
(*k*) Water loss after boiling period	.062 galls.	.05 galls.
(*l*) Steam and spray not recovered during whole run	.656 galls.	.25 galls.
(*m*) Duration of boiling period ...	20 secs.	20 secs.
(*n*) Maximum pressure in header tank during boiling period ...	1½ lbs.	1½ lbs.

It will be noted that the total water loss with the standard tank, item (*j*), was 2.9 gallons and that an excessive amount of water was lost before the thermometer in the header tank registered 100°C., item (*f*). This was due to the position of the thermometer, boiling was actually taking place in some portion of the engine when the thermometer was showing a temperature of 89.5°C., item (*g*). This accounts for the loss of water in flight; the pilot was unaware that the thermometer was not indicating the temperature of the hottest part of the system. If thermometers could have been in the cylinder jackets then the loss against item (*f*) would have been less and item (*i*) greater. The total loss, item (*j*), was considered excessive and was no doubt due to the small air space, item (*b*). The vent also was too small, as indicated by the pressure in the tank, item (*n*).

A new tank was fitted having a total capacity of 3 gallons and an air space of 1.47 gallons, whereas the original tank was of only 2.13 gallons total capactiy and 0.75 gallons air space. The results of this test are also given in Table I. Here again the water was boiling before the thermometer showed 100°C.; the loss of water, however, before boiling occurred was reduced to 0.75 gallons, item (*f*); item (*i*) was reduced to 0.05 gallons and the total loss, item (*j*), reduced to 1.1 gallons.

No improvement was noted in the pressure in the header tank, the vent pipe was therefore again increased in bore. The latter test was described to draw attention to the fact that not only is it important to fit the correct size of radiator, but also to determine by tests what water losses are likely to occur, for the aircraft cannot be replenished as easily as the motor car.

Petrol Systems

It would be thought that petrol systems have little effect on the behaviour of engines in flight, but were the systems as simple as those of the motor car, then little could be said. They are, however, much more complicated owing to the necessity of alternative methods of supplying the engine to defeat enemy action, and further the range of petrol heads varies greatly from machine to machine.

Unfortunately, all carburettor float mechanisms are not trustworthy, and it has been found that in an aircraft with differing pressure-heads or in aircraft having two methods of supply with different pressure-heads, the petrol consumption has been greater where the pressure has been the higher, thereby causing inconsistencies in fuel consumption test and in the consumptions of machines in squadrons. In no installation may the petrol heads be outside the range to which the carburettors are tested, *i.e.* 10½in. to 16ft.

The main test of the system is the measurement of the petrol flow at the carburettor union, when gravity fed, under the condition of minimum head obtainable in flight, and it is the rule to have a flow of 100 per cent. in excess of the requirement of the engine at full throttle at sea level.

Broadly speaking, the systems can be divided into two classes: (*a*) where where the feed is entirely by gravity, the alternative sources of supply being obtained by having more than one gravity tank; (*b*) where the feed is by pump under pressure and the alternative supply from a gravity tank.

Fig. 6 is an illustration of a system of the type mentioned under heading (*b*). In this case two main tanks are provided from which the petrol can drawn by the windmill pump either separately or together, and pumped through the piping and cock (*A*) to the carburettor. A pressure-relief valve set at 2½lbs. per sq. in. controls the pressure. The fourth method of feed is from the gravity tank through the cock (*A*) to the carburettor. A hand pump is installed to fill the gravity tank on the ground and in case the windmill pump fails in flight.

There is, however, a fifth method of supplying the carburettor, but this only arises because it is advisable to relieve the pilot of any form of manual work in flight. If during a flight on pressure-feed it is thought that the gravity tank is empty, then the cock (*A*) can be placed in such a position that the windmill pump will supply both the carburettor and gravity tank simultaneously, the excess overflowing back to the main tank.

The pressures obtained in the three cases are approximately as follows:—

Windmill pump, pressure, feed from either or both tanks, 2½lbs. per sq. in.
Windmill pump feeding carburettor and gravity tank, 1¼lbs. per sq. in.
Gravity feed only, 1lb. per sq. in.

It is this kind of system which has given rise to variations in consumption both on test work and in machines of a squadron.

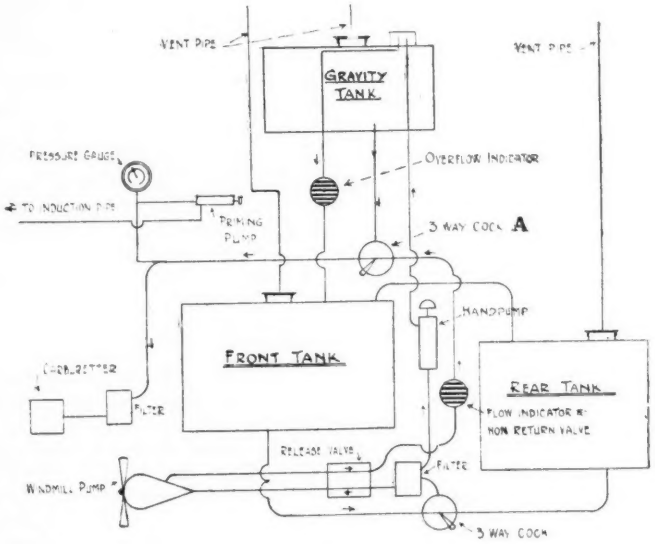


FIG. 6.

The various fittings necessary to the system, such as non-return valves, filters, release valves, etc., are always a potential source of trouble, and there is little doubt that greater reliability is obtained from gravity systems. Unfortunately it is not always possible to obtain sufficient gravity head from tanks in the body, so they are often placed on the top wing. Opinion is divided as to whether there is less risk of fire with pressure systems having tanks in the fuselage than with wing tanks.

With so many pipes and fittings and the necessity of avoiding other equipment in the aircraft, difficulty has been experienced in designing systems entirely free from air-locks, vent pipes being necessary in some cases to allow the air to escape before starting up.

The system whereby petrol is either forced direct to the engine, or to the gravity tank and thence to the engine, by compressed air applied to the surface of the petrol, was abandoned some years ago, for military reasons.

Considering from all points of view, the gravity system is best in that the head is practically constant, there are fewer fittings and shorter pipes and therefore greater reliability. It is, however, admitted that sufficient head is often not obtainable from fuselage tanks, and that tanks outside the fuselage are not conducive to cleanliness of aircraft design.

Ignition Systems

The problems of ignition are very little different from those of the car, neglecting the higher duties of the engines and the special precautions taken to ensure no interference with wireless reception. The large drop in air density at high altitudes, however, does have its effect, sometimes causing the safety spark gap in the magnetos to come into operation and so prevent firing at the sparking-plug points. Magneto ignition is favoured in preference to coil ignition, as the risk of fire on crashing is appreciably reduced.

Carburettors and Carburation

It will be appreciated that as the subject of carburation is so vast, particularly in flight, it is not possible to support all the different phases by test results. A measure of the effects only is given in the less important cases.

Carburettors of aero engines are very similar to those of cars and in some cases are enlarged and developed car carburettors; but provided with a device for the correction of the mixture strength at height, the petrol/air ratio being upset by the decrease in air density with increase in altitude. The barometric and temperature changes in flight are of a large order and affect carburation more seriously than any other part of the power plant. As stated earlier in the paper, the engine manufacturer tunes the engine to give the best performance under all the conditions obtainable on the test bed. It is not possible, however, to reproduce the conditions obtaining in aircraft.

For some years, it was necessary to retune engines upon installation in aircraft owing to the altered air intake conditions, chiefly through the use, for fire prevention reasons, of intakes of which the inlets were outside the engine bay, and the change in the bench jet setting often amounted to as much as 10 per cent. Further, a standard flight setting could not be adopted owing to the intakes varying with the type of machine. A great deal of development work has taken place during the last few years and to a very large extent this de-tuning has been eliminated by better balancing of the carburettors and the adoption of a standard intake for each type of engine. Although on cars air intakes are usually a part of the carburettor, it is believed that a change of tune occurs upon the transference of a car engine from the bench, which suggests that the methods of balancing aero engine carburettors could be adopted with advantage. At the present moment, the conditions in the float chamber are liable to be very different from those in the intake and being impressed on the jets, and doubtless this difference again varies from that obtaining on the test bed.

One aero engine still in use but designed during the War, has its standard gauzed covered intakes inside the engine bay, and seldom requires adjustment upon installation; it is therefore believed that with the improvement in the stability of carburettors it will be possible to revert to intakes in the engine bay, which in turn will provide better "opening up" at high altitudes through the aspiration of warmer air.

Before proceeding with the deatils of some of the carburettors, their behaviour in aircraft and the methods of testing, effects on carburation produced by flying at high altitudes may now be described.

Effect on Carburation of High Altitude Flying

The reduction of air temperature at height creates many problems, some of which have not yet been solved, and all of them become more pronounced as aircraft performance improves.

Much of the flight testing of engines during the last five years has been between the heights of 20,000 and 30,000 feet, and many expedients have had to be adopted to enable tests to proceed. It is not thought that the difficulties are realised to the full as there seems to be a tendency to be satisfied with obtaining a good performance on the test bed.

Generally it is possible to make continuous full-throttle climbs to a height without difficulty, but immediately upon closing the throttle and gliding, the rapid cooling of the power plant does not permit opening up of the engine and normal running until air temperatures such as are obtainable at 15,000 feet are again reached.

The various factors influencing carburation at height are briefly described under the following headings :—

(*a*) Effect of air temperature on fuel.
(*b*) ,, ,, on the fuel system.
(*c*) ,, ,, on the functioning of the carburettor.
(*d*) ,, position of carburettor and air intake.
(*e*) ,, low air temperature on distribution.

(*a*) An analysis of the upper air temperatures recorded during the last five years shows that the following temperatures are frequently to be obtained during the winter :—

5,000ft. – 3.5°C.	15,000ft. – 21.5°C.	25,000ft. – 41.5°C.
10,000ft. – 12°C.	20,000ft. – 31.5°C.	30,000ft. – 52°C.

These low temperatures suggest that some of the difficulties may be attributable to the fuel. Aviation spirit freezes at a temperature much below those experienced, but the addition of benzol for purely engine reasons, undoubtedly leads to difficulties, since benzol alone may freeze between –3.5°C. and –15°C., depending on its quality. In tests of 80 per cent. aviation spirit and 20 per cent. benzol a faint opalescence occurred at –15°C. No further change occurred until –56°C., to –58°C., when crystals began to separate. This portion apparently consisted of benzol, since the bulk of the liquid did not freeze when cooled to –70°C. The crystals disappeared again when the liquid reached a temperature of –52°C.

(*b*) More difficulty with fuel systems has been experienced in aircraft having wing tanks than with those having body tanks, not only because of the chilling of the fuel in the tanks, but because it is almost impossible to avoid feed pipes exposed to the slipstream. Reference to the effect of low air temperatures on fuel and to the table of air temperatures at high altitudes indicates that it is quite possible that choking of these pipes occurs due to the formation of benzol crystals, especially if a leak develops and local evaporation takes place. Even if body tanks are employed, it might be advisable to employ lagged tanks, to permit flying above 25,000 feet for periods greater than an hour.

(*c*) Although carburettors have improved in many other directions, and more heating has been applied to the carburettor itself and to other parts of the induction system, little has been done to render the diffusers immune from freezing and/or choking.

In the majority of old type carburettors and indeed in one or two of the present-day carburettors, there are diffusers which are particularly prone to freezing or choking especially on humid days, such that aircraft are often unable to fly at even low operational heights. This is caused through the diffuser

being isolated in the choke air stream, where there is a drop in pressure and in temperature, the latter due to the latent heat of vaporisation, and no conducted heat reaching the diffuser because of the small arms with which it is suspended in the choke.

To take as an example the old type Claudel Hobson diffuser in which there is a row of holes at the top for the emission of the petrol/air emulsion into the choke, the choking of these would completely stop the fuel flow and might be caused by (*a*) the formation of ice due to the freezing of deposited water; (*b*) the formation of benzol crystals. A row of holes at the bottom, for the ingress of air to the diffuser, would result, if choked, in a rich mixture, which only at low altitudes can be cured by applying altitude control. Other carburettors cease to function for similar reasons.

Chokes actually incorporating the emulsion holes have been tried, but they are sluggish in snap accelerations, as the speed of the air stream is lowest at the walls of the choke.

Since it appears necessary to have some device to intensify the suction on the jets or to place the emission in the centre of the air stream to give desirable characteristics, its connexion to the choke must be of ample dimensions to allow a large proportion of the heat applied to the carburettor body to be transmitted. The heating of the whole body of the carburettor is essential, not only to avoid freezing of the diffuser but to prevent the formation of ice on butterfly throttles or the seizing of throttle barrels due to contraction of the body of the carburettor. In design the carburettor might almost be considered as a heater-box in which the essentials of a carburettor have been fitted.

(*d*) The position of carburettors is very important, but in water-cooled engines it is not usually possible to place them where they can be easily heated without creating inaccessibility. This is not so in the case of air-cooled engines. In both types of engine the back of the engine is preferable.

The position of the air-intake inlets is also very important since those which have their inlets outside the engine bay, not only give no assistance to efficient atomisation, but are liable to collect snow in England or draw in sand in tropical countries.

In addition to the application of heat to the carburettor itself, the aspiration of warm air is essential as it tends to prevent the formation of ice. It can be collected either from the surfaces of air-cooled cylinders, the exhaust pipes, or with suitable carburettors, from engine bay. Even with external intakes, better use of the hot parts of the engine can be made than at present. It is considered that the arguments are greatly in favour of internal intakes, integral with the engine, for not only do they permit the aspiration of warm air and improve opening up at height, but promote consistency of power and consumption between test bed and aircraft, and between different types of aircraft.

The collection of warm air from the cylinders or exhaust pipes, however, is not possible on the glide, when it is most needed. The aspiration of air warmer than the surrounding atmosphere is possible from the engine bay, and coupled with adequate heating of the carburettor, improvement upon the opening up at present obtainable should result. It is hardly conceivable that it will ever be possible to descend from, say, 35,000 feet to 25,000 feet and then obtain good opening up except by generating sufficient heat by running the engine at 1,000 revs. per minute during the descent.

(*e*) Distribution involves the whole of the induction system. In water-cooled engines it is invariably the practice to water-jacket the carburettor and the whole of the induction pipes, but in air-cooled engines oil heating of the carburettor coupled with oil and/or exhaust heaters between the carburettor and the crankcase are general practice, but the distribution pipes from the crank-

case are usually unheated, a feature that should be remedied at the earliest possible moment.

Exhaust heating fails when throttled for gliding and the effect of water heating is reduced, as the radiator shutters are seldom sufficiently air-tight, therefore the water loses its heat much more rapidly than the oil. Oil heating of the carburettor and induction system of air-cooled engines is strongly recommended, even including the diffusers, but on account of the simplicity it is thought desirable to retain water heating of the induction pipes of water-cooled engines.

Balancing and Altitude Control

The foregoing has dealt with the general difficulties encountered with carburation when flying high, and refers to the necessity of retuning for flight. In the detailed description of the functioning of types of carburettors that follows the causes of their de-tuning are explained and later the newer types

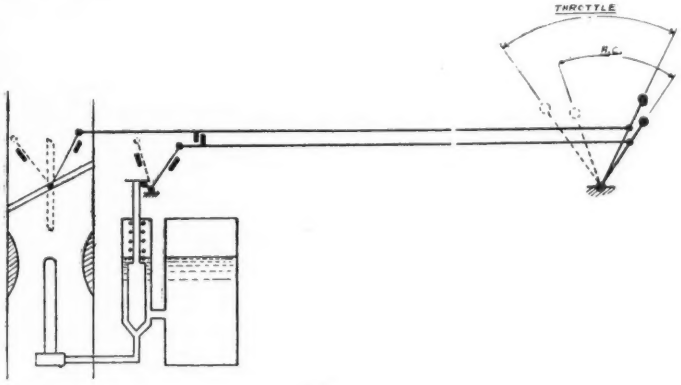


FIG. 7.

of carburettors from which these vices have been eliminated are described. The device known as the altitude control, provided to permit the pilot to maintain the petrol/air ratio by weight practically constant at all altitudes, usually consists of an arrangement whereby the fuel flowing from the jets can be reduced and is operated by a lever adjacent to that of the throttle. The arrangement used to be designed to enable a reduction of 35 per cent. in fuel flow to be obtained on the test bed, but this has now been raised to 50 per cent. to suit the requirements of modern high-altitude aircraft. In order to ensure a rich mixture for "opening up," however, the altitude control device is closed automatically at height, when the throttle is closed. The gear provided to achieve this is usually on the carburettor itself and frequently consists of a projection on the altitude control lever and an adjustable screw in the throttle lever, the screw making contact with the projection when closing the throttle. A slight movement of the throttle from the fully-opened position is permitted with full altitude control before the throttle lever engages with the altitude control lever; Fig. 7 clearly illustrates this.

In all carburettors attempts are made to balance the conditions prevailing in the air intake with those in the float chamber and the region of the jets, and the manner of achieving this is given in the following description of carburetttor types.

Types of Carburettors

Fig. 8 shows diagrammatically and explains briefly the features of three types of carburettors that have been in use.

In the arrangement of the old type Claudel Hobson H.C.8 carburettor, once fitted to the Jupiter engines, it will be seen that the balance pipe is badly situated, is open ended, and has two functions to perform, (*a*) transference of the air intake conditions to the float chamber, and (*b*) provision of an air-leak to lessen the effect of the choke depression on the float chamber when the altitude control cock is opened. To fulfil condition (*a*) it is essential that the balance pipe be as large as possible for rapid transference of changes in air intake conditions. To fulfil (*b*) the size of the pipe should be small and vary with each installation to provide sufficient control for the ceiling of the machine, and should vary also to adjust the differences in leakage along the surface of the cock due to variations of fit.

The bad balance pipe entry results in a higher pressure in the carburettor float chamber than in the intake, the recorded pressure-difference of 0.015lbs. per sq. in. resulting in an increase of flow from 200 to 210lbs. per hour and explains why engines with this carburettor usually require a reduction in jet size for flight of 780 c.c. to 680 c.c. per minute.

If the altitude control cock is set for full throttle at any given height the depression in the float chamber is reduced as the throttle is closed, altering the effect of the altitude control, irrespective of the returning of the control to the closed position by the throttle lever. The balancing and altitude control arrangement should be independent, so that the effectiveness of the control is independent of the throttle opening.

To provide a 50 per cent. reduction in fuel flow on the ground by closing up the balance pipe orifice, to suit the modern high-altitude aircraft, would render the balancing extremely sluggish during aerobatics.

The Rolls-Royce Claudel Hobson carburettor as fitted to the Falcon engine, although of the Claudel Hobson type, differs somewhat from that already described. The altitude control is independent of the balancing and is obtained by interposing a variable fuel cock between the float chamber and main jet diffusers. This variable fuel cock is also used for tuning on the bench. The effectiveness of the control in this case is unaffected by the throttle position. The balance pipe entry surrounds the air inlet holes to the main jet diffusers, to transfer the changes in air intake conditions simultaneously to the diffuser and to the float chamber. The actual entry, however, is a hole approximately $\frac{3}{32}$in. in diameter, and, as will be seen from the sketch, faces the direction of air flow. The pressure is therefore higher in the float chamber and tends to give rise to richness.

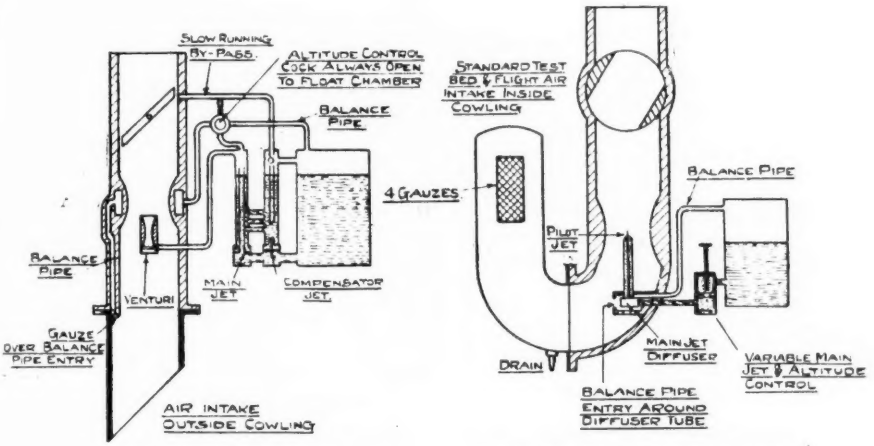
The carburation of engines fitted with this carburettor, however, is found to vary very little from test bed to aircraft and in different types of aircraft, due undoubtedly to the standardisation of the air intake for both test bed and flight. This shape of intake is strongly recommended, as the drain is at the bottom and remote from the actual air entry, which is covered with gauze. Synchronisation of several carburettors is comparatively easy.

The carburettors on the Condor engines are of a similar type and show a noticeable tendency to become richer when installed in an aircraft, but air intakes with inlets outside the engine bay and in the slipstream of the airscrew are employed in this case.

The Zenith carburettor, also illustrated in Fig. 8, once used on Jaguar engines, suffered from similar defects, the bad position of the balance pipe causing a higher pressure in the float chamber, the difference being 0.048lbs. per sq. in., and resulting in an increase of fuel flow on the ground of from 168 to 180lbs.

per hour. Although this difference in pressure is greater than in the Claudel Hobson H.C.8 carburettor it is somewhat mitigated by the larger difference in pressure between float chamber and choke, on account of the double venturi. The pressure-difference between choke and float chamber in the case of the H.C.8 carburettor was 0.64lbs. per sq. in., whilst the difference between the main jet compartment and the float chamber on the Zenith was 0.91lbs. per sq. in.

The balancing arrangements are again poor as the balance pipe has to perform two functions, and the altitude control is therefore not independent. The effectiveness of the control is only slightly altered as the throttle is closed



ZENITH .55 DC. CARB AS USED ON JAGUAR IV ENGINES

NOTE.—Balance pipe, bad entry and position. Float chamber pressure higher than air intake, altitude control and balancing dependent. Pipe serves as air leak and balance pipe carburation only slightly affected upon installation by non-standardisation of air intake. Effectiveness of altitude control only slightly affected as the throttle is closed. Synchronisation of several carbs difficult.

ROLLS-ROYCE CLAUDEL-HOBSON CARBURETTER AS USED ON FALCON & EAGLE

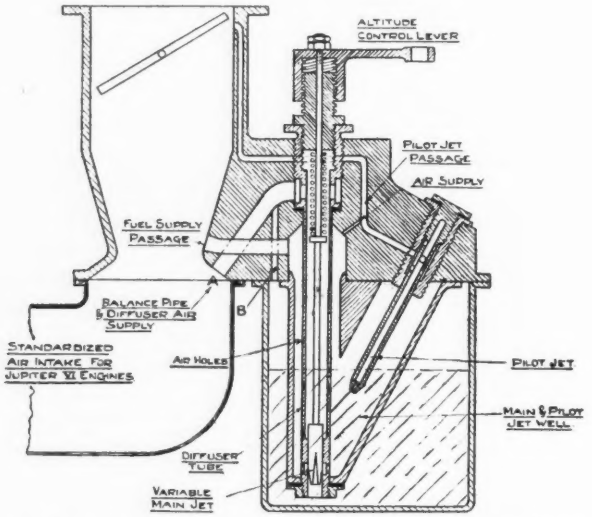
NOTE.—Balance pipe entry faces airstream, pressure in float chamber therefore higher than that in air intake. Altitude control and balancing independent. Carburation unaffected upon installation owing to standardisation of air intake. Effectiveness of altitude control unaffected as the throttle is closed. Synchronisation of several carbs simple.

FIG. 8.

towards the point where the altitude control lever is engaged by the throttle, since it is obtained by introducing air at float-chamber pressure into the main jet compartment, tending to destroy the suction on the jet. The control, however, is somewhat coarse in action. Synchronisation of several carburettors is difficult because of differences in leakage on the small dimensioned control cocks.

The balancing and altitude control arrangements of the three carburettors just described represent the three most popular methods of providing a device for adjustment of mixture strength, but all three carburettors are obsolescent, and have been superseded on the Jupiter and Jaguar engines by modern carburettors, the designers of which have put forward their best efforts to

eliminate the vices of the forerunners and produce really well balanced and flexible carburettors. Extraordinary efforts have been made not only to ensure that they are unaffected by transference to an aircraft, but also that there should be little variation between individual carburettors to lessen the difficulties of synchronisation.



BRISTOL TRIPLEX CARBURETTOR AS USED ON JUPITER VI. ENGINES.

NOTE.—Balance pipe entry "A" is not well positioned, but this is alleviated by the size and position of the small hole "B" communicating with float chamber. The balancing is independent of other functions, such as altitude control and fuel supply. Effectiveness of altitude control unaffected by throttle position. Carburation unaffected upon installation. Synchronisation of several carbs simple.

FIG. 9.

The Bristol Aeroplane Company introduced the Triplex carburettor for Jupiter engines in 1924, with an air intake integral with the engine. It really comprises three carburettors with one common float chamber, to suit their special threefold induction system, and to ensure that a "pop-back" does not cut out the whole engine; the air intake also is divided into three. Fig. 9 is a diagrammatic section through an early model of the carburettor, but is not quite up to date, the fuel supply passage is not used now as the balance pipe, the emulsion being emitted into the chokes by 60 1/16in. holes in the choke walls, and balancing is via an extra passage and the 36 1/16in. holes in the base of the detachable choke.

The body of the carburettor and the air intake are heated by oil; the air intake collects warm air from the cylinders and the induction pipe is heated by exhaust gases; this latter, however, has been deleted in later models in favour of more complete heating of the carburettor body by oil and an oil-heated box between crankcase and carburettor. Tests show that the balancing

is not disturbed and the difference in fuel flow is hardly measurable when tested first with an air intake having a pressure greater than atmospheric, and secondly with an air intake having a depression. The altitude control is provided by three interconnected variable jets, and is independent of the balancing arrangements; synchronisation of several carburettors and of the three parts of the carburettor has been found to be quite simple, chiefly due to the methods of manufacture and inspection. The general behaviour of the carburettor is good and it is a pleasure to handle, even at high altitudes.

Messrs. Armstrong Siddeley also have provided their engine with a new type of carburettor, the A.V.T.70, which has been designed and tested with the same care and objects in view by Messrs. H. M. Hobson, Ltd., and extremely good results have been obtained.

It is a twin-choke carburettor employing the system of altitude control, wherein the depression in the main jet compartment is relieved by the introduction of air from the balance pipe. The arrangement is such that a very large and well-positioned balance pipe can be employed. The balancing, is, therefore, practically unaffected by the use of altitude control, although not independent. It retains in a modified form the well known Claudel Hobson diffuser arrangement and is provided with a standard intake. The body of the carburettor is oil heated, as is also the induction pipe between carburettor and crankcase by means of a special heater box. Power jets are provided in order to promote economy when cruising.

Fig. 10 is a diagrammatic sketch of the first experimental type flight tested. The passing of the fuel from the pilot jet to the butterfly has been abandoned, as it was too difficult to reproduce with any consistency; the acorn diffusers have been exchanged for short copper pipes cut off at 45° in the centre of the choke, to eliminate freezing or choking.

The carburettor was primarily designed for supercharged engines, which have their own peculiar problems, though it has been successfully employed on straight engines, by the elimination of the trailing second throttle barrel, as it is not required to maintain the weight of air constant with increase of height.

Factors Affecting Economy in Flight

The problem of economy is of great interest to both those on the ground and in the air, and the factors affecting this in the air can best be studied under two headings, "full throttle" and "cruising."

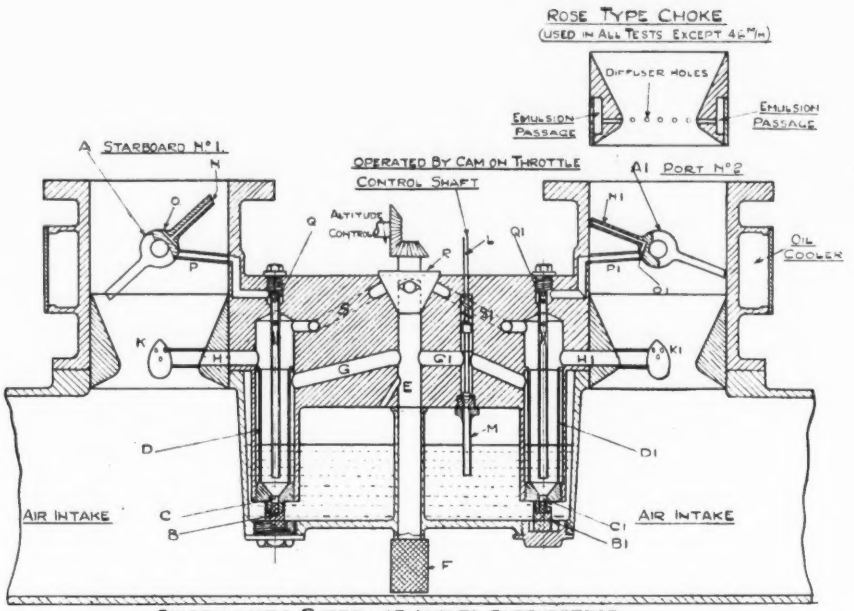
It has been stated previously that the engine manufacturer has tuned the engine to give optimum performance and economy at full throttle, and it has been shown what steps are taken in carburettor design to ensure that this is still obtained in the aircraft on the ground.

As an aircraft gains height, the pilot is expected to operate the altitude control, and to maintain economy and performance, but this is a little difficult when it is remembered that an engine tends to become rich with increase in height and that the falling off in power with richness is not very marked.

The band of consumptions over which there is only a small change in power on the test bed, is widened considerably in flight due to the revolution indicator being a poor indicator of incipient falling off of power. With the use of a fuel flowmeter it has been shown that the consumption may be varied 20 per cent. from weak to rich, without any apparent change in power.

Fig. 11, a calibration of an early Triplex carburettor, in addition to showing the band of consumptions obtainable with the use of altitude control without any apparent change in revs. per minute or power, shows also the consumption without altitude control and the full range of control available at 20,000 feet.

A Butterfly Valve.
B Gauze.
C Main Jet.
D Diffuser Tube.
E Air Balance Pipe.
F Gauze for *E*.
G Diffuser Air Passage.
H Emulsion Passage.
K Diffuser Cone.
L Power Jet Needle.
M Power Jet.
N Hole.
O Hole.
P Slow Running Tube.
Q Slow Running Jet & Tube
R Altitude Control Cock.
S A.C. Passage to Diffuser.



DIAGRAMMATIC SKETCH OF AVT 70 CARBURETTOR

FIG 10.

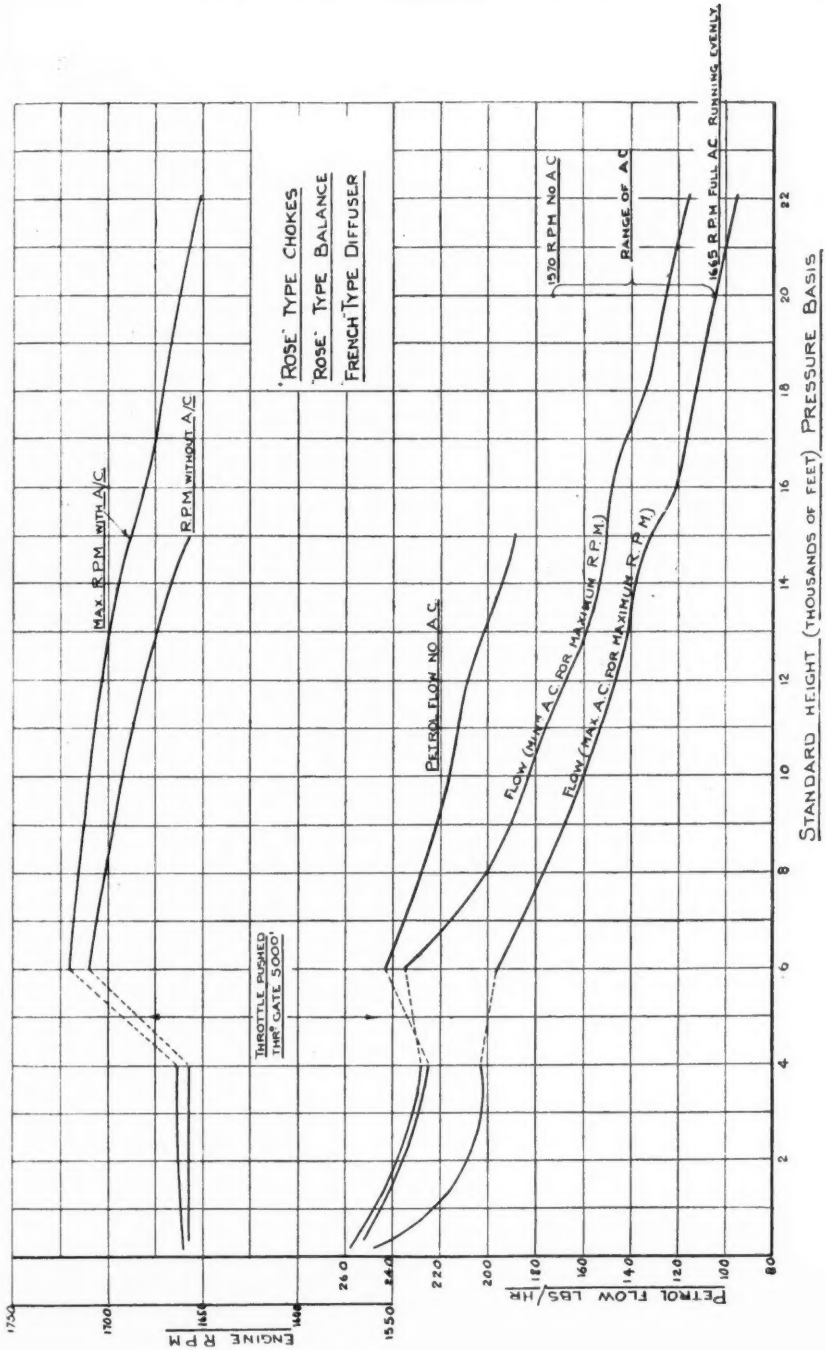

'ROSE' TYPE CHOKES
'ROSE' TYPE BALANCE
FRENCH TYPE DIFFUSER
MAX. R.P.M. WITH A/C.
R.P.M. WITHOUT A/C.
THROTTLE PUSHED THRU' GATE 5000'
PETROL FLOW NO A.C.
FLOW (WITH A.C. FOR MAXIMUM R.P.M.)
FLOW (MAX. A.C. FOR MAXIMUM R.P.M.)
1370 R.P.M. NO A.C.
RANGE OF A.C.
1465 R.P.M. FULL A.C. RUNNING EVENLY
ENGINE R.P.M.
PETROL FLOW LBS/HR
STANDARD HEIGHT (THOUSANDS OF FEET) PRESSURE BASIS

FIG. 11.

That particular model of carburettor was tuned a little too rich, and had not quite sufficient altitude control; subsequent models were improved in this respect. The pilot, therefore, can obtain flows of any value that lie inside the band and yet apparently be developing full power.

Aircraft at present are not provided with fuel flowmeters, thus making the judging of the correct amount of altitude control difficult, with the result that consumptions outside this band are often obtained and have been known to differ as much as 40 per cent. on the same type of aircraft.

It is well known that power falls very rapidly with weak mixtures and since the point at which power falls is very clearly defined in flight, in order to prevent even the 20 per cent. variation, maximum altitude control without loss of revs. per minute has been adopted as a standard. Tests by a number of test pilots indicate that their interpretation of maximum altitude control, that is minimum consumption, varies less than 2 per cent.

Recent tests, to demonstrate the consistency obtained in squadrons by insisting on the use of the maximum altitude control, and providing machines with flowmeters, shows that the consumption can be controlled within 15 per cent., even including the variation due to age of engine and position of a machine in a formation. The saving of fuel should be very marked, and the task of the squadron leader made easier, in that the range of the aircraft under his command can be predicted with fair accuracy. Further, this method of controlling the mixture strength should enable him to determine, as data are collected, the most economical height and speed at which to fly, and in any case, it should prevent machines of a formation falling out through having used all their fuel before others. It is even probable that the squadron leader will be able to declare to the other pilots the fuel flow for a particular flight either in the air at the beginning of the flight, or previously on the ground.

The foregoing applies mainly to full-throttle flying, but when an engine is throttled for cruising, additional difficulties arise and it is by no means certain that the minimum consumption is obtained.

Not only is it necessary for the engine manufacturer to obtain minimum consumption at full throttle, but also at all points on the throttle consumption curve down to, say, half throttle.

This has recently been explored on the ground, but on account of the difficulties of determining accurately the power absorbed by an airscrew when running on the ground, specific consumptions cannot be given. For the purposes of the test the altitude control lever and throttle lever were not interconnected.

Table II. indicates that the carburettors are well tuned at full throttle, and that they become rich and therefore require more altitude control as the throttle

TABLE II.

Bristol Triplex Carburettor. Jupiter VI. Engine. 5·3/1 Comp. Ratio.					A.V.T.70 Carburettor. Jaguar Engine.				
	Petrol Flow lbs./hour.			% of A.C. Needle		Petrol Flow lbs./hour.			% of A.C. Cock
R.P.M.	No.	A.C.	Max.	A.C. Movement.	R.P.M.	No.	A.C.	Max.	A.C. Movement.
1640 (F.T.)	250		240	20.2	1780	225		220	26
1600	240		197·5	20.2	1700	205		190	41.8
1400	170		132.5	30.8	1600	173		160	55·5
1200	112.5		90.0	56.9	1400	130		100	73·5

is closed, which is a great disability. Imagine for the moment that a pilot is flying with the altitude control disconnected from the throttle return gear, so that he can obtain any desired amount of control, it will be necessary for him,

in addition to adjusting it correctly for full throttle at any height, to adjust for every throttle opening at that height.

Each carburettor manufacturer has his own particular method of determining that part of the throttle consumption curve controlled by the main jets.

In the Condor and the A.V.T.70 carburettors the disposition of the air holes in the diffuser tubes determines the middle portion of the curve, but the former employs an air port cut in the side of the throttle barrel and opens towards full throttle whilst the latter brings into operation power jets. The Triplex curve is determined throughout by the disposition of the air holes in the diffuser tube.

Figs. 12 and 13 show calibrations in flight, whilst climbing, of the A.V.T.70 and Condor carburettors respectively. The former confirms the ground tests Table II. and the Condor flight tests show the same tendency, *i.e.*, increase in consumption, and therefore in the amount of altitude control required as the throttle is closed.

In the interests of economy and also of the pilot, the throttle consumption curve should be at a minimum from full throttle to the lowest cruising speed, and require no adjustment of the altitude control throughout the range.

All these tests were conducted with the control disconnected from the throttle, but when the normal interconnection is introduced, as depicted in Fig. 7, further difficulties arise in some makes of carburettor. The point at which the altitude control is engaged by the throttle, when closing, varies considerably with the make of carburettor. For the three carburettors under discussion the amount of throttle movement before it is engaged is with the Condor, 41 per cent., with the A.V.T.70, 40.5 per cent., and with the Triplex, 2 per cent.

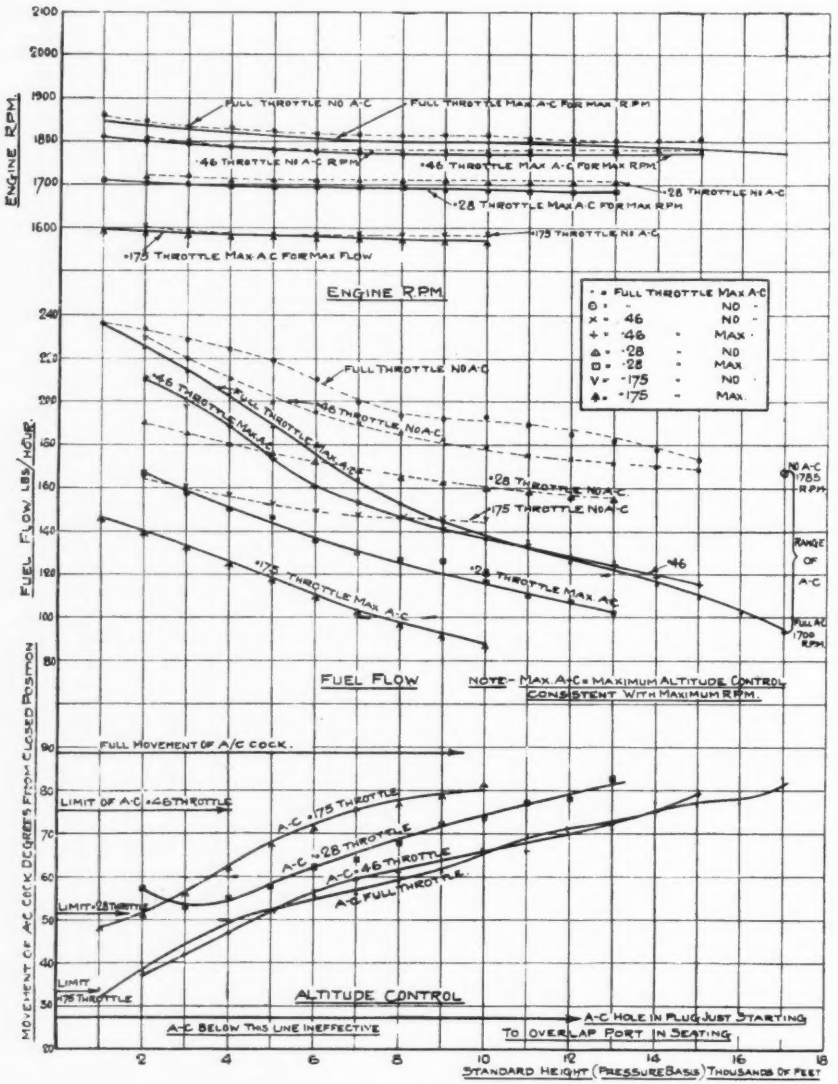
The Triplex only permits sufficient freedom to ensure that both full throttle and full mixture control can be obtained without interference one from the other. The A.V.T.70 carburettor was given the large movement of 40.5 per cent., as it was thought that no adjustment was required with change of throttle position, and to leave the pilot free to fly over the large range of speeds obtained from modern aircraft without adjustments. The particular aircraft used for the tests in Fig. 12 had a very large cruising speed range, *i.e.*, from 50 to 140 miles per hour.

Reference to Fig. 12 will show that with the interconnection in operation, at full throttle the maximum altitude control is not required until approximately 10,000 feet, but at 0.46 throttle at above 11,000 feet insufficient control is obtainable, whereas at the 0.28 and 0.175 throttle positions at no height is it possible to obtain sufficient, the control being limited by the throttle position. Further, supposing a pilot has obtained the correct altitude control for full throttle at any given height, and the throttle is gradually closed, the control has to be increased until the point is reached when the altitude control is closed automatically by the throttle. Failure to adjust the control, and its subsequent automatic closing, both result in fuel wastage.

From the pilot's point of view, the system is unsatisfactory, for each time he partly closes the throttle beyond this point he loses the correct control position for full throttle and has frequently to re-determine it. The Condor carburettor calibration, Fig. 13, however, indicates that at no position of the throttle from medium cruising to full throttle speeds, is the control interfered with by the throttle position, although it is necessary to increase the control as the throttle is closed.

It should be stated that it was only until recently that these exhaustive tests were commenced, and were initiated with the object of providing the carburettor manufacturers with sufficient data to allow them to overcome the known difficulties, and to arrive eventually at a state where carburettors are automatic

and set the mixture strength correct not only for change of height but also for change of throttle position at any given height. The former might be met by an aneroid control, the latter is more difficult of accomplishment, but the


ENGINE R.P.M.
FULL THROTTLE NO A-C
FULL THROTTLE MAX. A-C FOR MAX. R.P.M.
·46 THROTTLE NO A-C R.P.M.
·46 THROTTLE MAX A-C FOR MAX R.P.M.
·28 THROTTLE NO A-C
·28 THROTTLE MAX A-C FOR MAX R.P.M.
·175 THROTTLE NO A-C
·175 THROTTLE MAX A-C FOR MAX FLOW
ENGINE R.P.M.
· - FULL THROTTLE MAX A-C
○ - NO
× - ·46 NO
+ - ·46 MAX
△ - ·28 NO
□ - ·28 MAX
▽ - ·175 NO
✦ - ·175 MAX
FUEL FLOW LBS/HOUR
FULL THROTTLE NO A-C
·46 THROTTLE MAX A-C
·46 THROTTLE NO A-C
·28 THROTTLE NO A-C
·175 THROTTLE NO A-C
·175 THROTTLE MAX A-C
·28 THROTTLE MAX A-C
·46
NO A-C 1785 R.P.M.
RANGE OF A-C
FULL A-C 1700 R.P.M.
MAX. A-C 1785 R.P.M.
FUEL FLOW
NOTE- MAX. A-C = MAXIMUM ALTITUDE CONTROL CONSISTENT WITH MAXIMUM R.P.M.
MOVEMENT OF A-C COCK DEGREES FROM CLOSED POSITION
FULL MOVEMENT OF A/C COCK.
LIMIT OF A-C ·46 THROTTLE
LIMIT ·28 THROTTLE
LIMIT ·175 THROTTLE
A-C ·175 THROTTLE
A-C ·28 THROTTLE
A-C ·46 THROTTLE
A-C FULL THROTTLE
ALTITUDE CONTROL
A-C BELOW THIS LINE INEFFECTIVE
A-C HOLE IN PLUG JUST STARTING TO OVERLAP PORT IN SEATING
STANDARD HEIGHT (PRESSURE BASIS) THOUSANDS OF FEET

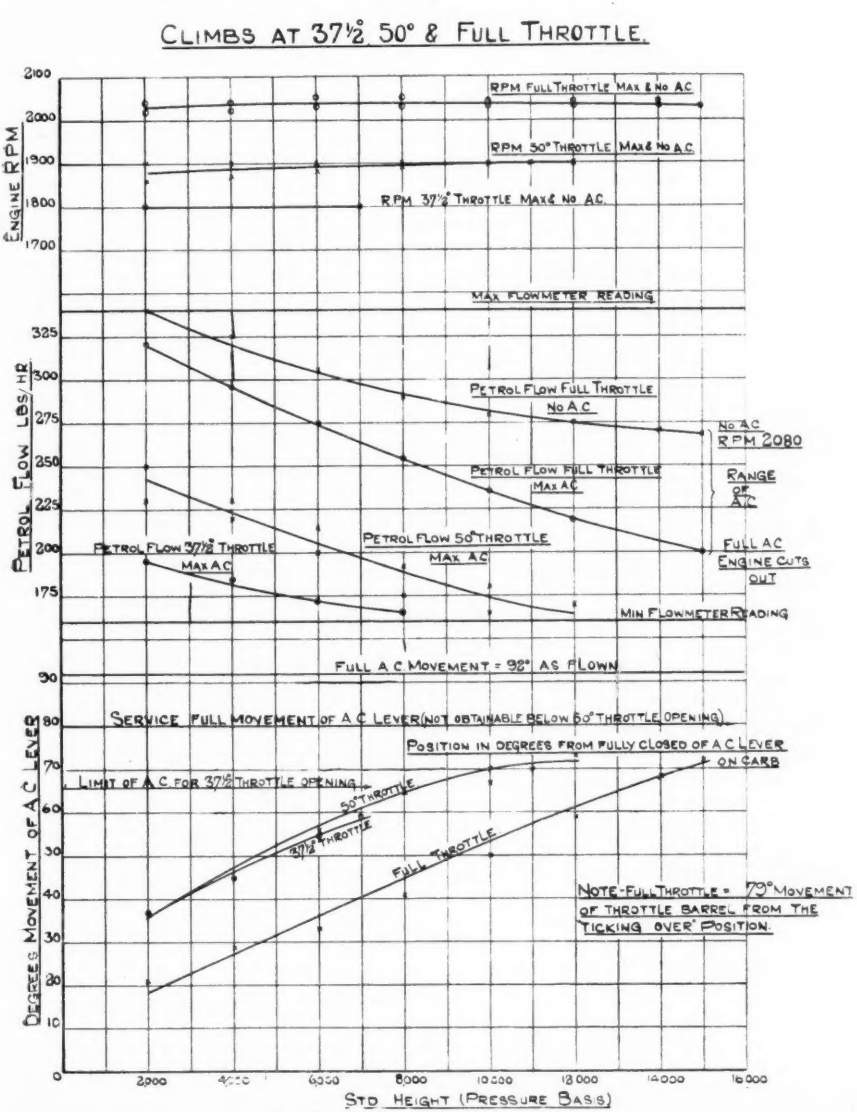
FUEL CONSUMPTION R.P.M. & ALTITUDE CONTROL
CLIMBING AT 70 M.P.H. INDICATED AIRSPEED

A.V.T. 70 Carburettor.

FIG. 12.

combination of the two is exceedingly difficult, and it will no doubt be some time before success is reached.

All the foregoing has to be accomplished without any effect upon the opening-up characteristics, and as the latter is becoming more and more troublesome as the ceiling of aircraft is increased, especially with supercharged engines,


CLIMBS AT 37½°, 50° & FULL THROTTLE.
ENGINE RPM
RPM FULL THROTTLE MAX & NO A.C.
RPM 50° THROTTLE MAX & NO A.C.
RPM 37½° THROTTLE MAX & NO A.C.
MAX FLOWMETER READING
PETROL FLOW LBS/HR
PETROL FLOW FULL THROTTLE NO A.C.
NO A.C. RPM 2080
PETROL FLOW FULL THROTTLE MAX A.C.
RANGE OF A.C.
PETROL FLOW 37½° THROTTLE MAX A.C.
PETROL FLOW 50° THROTTLE MAX A.C.
FULL A.C. ENGINE CUTS OUT
MIN FLOWMETER READING
FULL A.C. MOVEMENT = 98° AS FLOWN
SERVICE FULL MOVEMENT OF A.C. LEVER (NOT OBTAINABLE BELOW 50° THROTTLE OPENING)
POSITION IN DEGREES FROM FULLY CLOSED OF A.C. LEVER ON CARB
LIMIT OF A.C. FOR 37½° THROTTLE OPENING
50° THROTTLE
37½° THROTTLE
FULL THROTTLE
NOTE-FULL THROTTLE = 79° MOVEMENT OF THROTTLE BARREL FROM THE TICKING OVER POSITION.
DEGREES MOVEMENT OF A.C. LEVER
STD HEIGHT (PRESSURE BASIS)

Condor R.R.C-H. Carburettor.

FIG. 13.

special provisions will be necessary to maintain the standard obtaining at low altitudes.

The increase in the capacity of the induction systems is mainly responsible for the poor acceleration, and a large increase in the wells of the carburettor, or some means of wetting the walls of the induction system appears to be necessary since deposition of the fuel practically ceases and the walls become dry when throttled down. It would be inadvisable to say more on this subject at the moment as insufficient work has been done to determine the chief causes and remedies.

It is therefore urged that equal attention be given to raising to the desired standard, the important factors, ease of control, flexibility, rapid acceleration, as is given to the pursuit of power and economy. In the present designs of carburettors the latter is frequently in opposition to the former.

Discussion

Mr. A. H. R. Fedden, in opening the discussion, said: With regard to the oil system, can the author explain in a little more detail the relationship of oil temperature and oil circulation in respect of the oil cooler problem.

The speaker had been trying to introduce oil cleaners for some time, and although they might be looked on by the aircraft designers as another nuisance, he believed everyone would agree with the author's remarks as to their advantages.

Another important question is that of oil pipe breakages and forced landings through loss of oil; a good flexible connection for oil pipes is badly needed, and in addition, he was very keen upon having some form of oil gauge, such as the Amyot, in which the fracture of the connecting pipe does not involve the loss of oil. He had been informed by the head of one of the largest air transport companies that by the introduction of the Amyot system twenty forced landings had been avoided during the past two years.

In connection with the detailed work carried out by the R.A.E. in the development of radiators for water-cooled engines, he would like to draw the analogy that if such work is necessary on water-cooled installations, it is obvious that similar care and experimental work is required on air-cooled engines. He admitted that in the past sufficient care and attention had not been given to air-cooled engine installations to make sure by the use of suitable instruments that an adequate amount of air for cooling is given to each cylinder. He submitted that all new air-cooled installations should be tested on similar lines to those which have been outlined by the author for water-cooled installations.

In reference to the question of fixed or variable ignition, he considered that with supercharged engines, variable ignition must be used.

Certain modifications have been made to the Bristol Triplex carburettor as compared with the type described by the author, and by opening out the emulsion holes alternately, remarkably stabilised carburation is obtained, no matter what type of air in-take is used.

He attached great importance to placing the balancing arrangements in the neutral zone of the choke, especially for acrobatics.

He agreed with the author's remarks regarding benzol, because a great deal of trouble from freezing at altitudes has been experienced. His own view was that more than 20 per cent. of benzol should not be used, and this brought forward the question as to whether dope was not the thing to go for rather than a high benzol content, because of this serious freezing trouble.

The problem of opening up after a glide with very high altitude machines is a serious one, and perhaps the only thing to do is to scrap carburettors altogether, and he did not think that was such a rash remark as it might seem

at first sight. Indeed, he believed that in another four or five years' time we shall have high altitude, high performance machines, still using the petrol engine, but not using carburettors.

In conclusion, he hoped the author's department would develop a really suitable fuel flowmeter for flight and an automatic altitude control, because he regarded these components as very necessary.

Flight Lieutenant BULMAN: The author has taken the view that oil cooling systems are similar to water radiators in that the oil reaches its highest temperature at some height on a climb, but when flying at full throttle level for long periods near the ground, the oil temperature would certainly rise to a higher figure. Much information on the subject of oil cooling has been learned from long distance air racing, and engines carrying their oil in the sump have operated successfully under these conditions without external oil coolers. Can the author say which engines require oil coolers from which, in the design of the engine of the future, the causes of excessive heat being carried to the oil might be eliminated? The cleanest possible external form of the aeroplane could then be retained in this climate, the oil radiator only being used in hot countries.

Captain DODSON: Has the paper finished at the most thrilling period? He believed that recent work has gone a long way to realise the hopes expressed in the last paragraph. He did not know whether these improvements had yet been flight tested. The later developments were the results of the most hearty co operation and team work between the Air Ministry, Messrs. Armstrong-Siddeley and Messrs. H. M. Hobson. Without the extraordinary energy and help of Mr. Foord, of the Air Ministry, progress would have been very much less rapid. Messrs. Armstrong-Siddeley always seemed to have a bench and an engine available whenever Messrs. Hobson wished to do any carburation development work, and there was always a hearty welcome for them at the Royal Aircraft Establishment when it was required to extend these test bench experiments to flight. Mr. Davey would no doubt say something more about his part in the experiments on the bench at Farnborough, but by reason of the Official Secrets Act he would himself refrain from saying anything about the work that had been done since the compilation of the paper.

Mr. DAVEY: He did not agree with the author that water is the best medium for heating the induction systems on a water-cooled engine. It seemed to him that the dual advantage of oil heating was apparent on both air-cooled and water-cooled engines, as the oil cooling and the induction system heating are required on both types of engine; therefore, why not utilise the latent heat of vaporisation of the fuel to the maximum possible extent? With regard to altitude control, the early vacuum control seemed at that time the best type for use in aeroplanes, but the disadvantages are so apparent now that it surprised him to know that it was still being designed into at least one modern carburettor. The timing of the over-ride mechanism of altitude control should be arranged in the cockpit of the machine, first because it takes all the work off the rather light mechanism of the altitude control lever on the carburettor, and secondly, because it would be much simpler to arrange a longer free operating range. Dealing with the latest carburettor developed for Messrs. H. M. Hobson, Ltd., which he understood was the one which Mr. Penn had used for the tests in the early stages of development, the diffuser well was used as the only accelerating device, but since that time a separate means for dealing with this particular function has been incorporated, and this has enabled them to use the diffuser solely as a measuring device. Since the completion of the paper, curves have been taken which show that the throttle curve can be made such that it falls directly on to the optimum points at all positions of the throttle throughout its useful range, and even considerably lower, proving that the fault mentioned in the paper with regard to more altitude control being required at smaller throttle

openings, was not an actual altitude control fault, but due to the fact that it was necessary to have a badly over-corrected throttle curve in order to obtain good snap acceleration. He was confident that with their knowledge up to date it was possible to adjust a carburettor to give excellent snap acceleration, and a highly economical throttle curve which is bodily displaced parallel to its original form by the use of the altitude control to any desired extent. He was rather at a loss to understand the author's statement that "float gears" are being responsible for varying consumptions, provided that they were designed to come within the schedule of the Air Ministry. The only conclusion he could come to by Mr. Penn's remarks and his own personal observation was that many carburettors in service did not comply strictly with these requirements, although as regards modern carburettors, as a general rule they were strictly in accordance with the letter of the schedule laid down by the Air Ministry, which often raised a criticism as regards bulk, but as was realised by all, it was quite impossible to deal with both the large heads and the large flows without having an ample size of float, which necessarily increased the overall bulk of the carburettor. Dealing further with this point, Mr. Davey said that on many occasions he had recommended an intermediary float chamber for breaking down the large heads on some machines. This appeared the most obvious remedy both from making a satisfactory float gear and a very much smaller float chamber. Possibly the "powers that be" would bear this in mind.

Mr. A. F. Elliott: Was the carburettor used for the tests in Fig. 12 tuned prior to these tests for any particular property of carburation, such as maximum power or acceleration or for everything? Had Mr. Penn investigated the point as to whether the vibrations on the float chamber of the carburettor have any effect on the fuel consumption of the engine?

Mr. H. G. Burford: There appear to him to be three difficulties to be faced; the principal one is lubrication. Experience seems to show that when lubricating oil is at about a temperature of 100°F., it has reached the limit of usefulness as a lubricant; aeroplane engine designers should therefore, instead of using an ordinary sump, use the condenser type built up of sheet metal and controlled by a thermostat, so that complete control would be maintained over the oil and the temperature kept constant. The ordinary engine as designed to-day, has a maximum speed of about 3,000 revs. per minute with its complication of tappets, cams, etc. In the design of the aeroplane engine, in which money very often is not a serious consideration, it should be possible to put the camshaft straight on the valves and to do away with plain bearings and use ball bearings and so reduce friction losses. He believed that it would then be possible to attain speeds of 5,000 or 6,000 revs. per minute. The carburettor question is certainly a difficult one as compared with those used on the roads, but it seemed to him that the genius of the young men in the aircraft industry should find no difficulty in maintaining the temperature round the carburettor by means of an electric coil, used in conjunction with a thermostat, so that the coil could be switched off when necessary and the temperature kept constant.

Mr. A. E. Parnacott: Has the author obtained any results showing variations in petrol flow due to the vibration of the pipe? He agreed with Mr. Fedden as to the practicability of superseding the carburettor with something analagous to that on the so-called Diesel type of engine, not throttling the air charge to such a large extent, but pumping—similarly to the manner adopted on heavy oil engines for marine purposes—the petrol separately into each cylinder. If carburettors must be used then their form should be changed entirely; instead of trying by a complicated arrangement of orifices to proportion, as is after all required, the weight of fuel to the weight of air, an arrangement should be adopted so that the mass of air rather than the differences of pressure resulting from differences of air velocities, as at present, should be the factor governing

the fuel supply. Carburettors should, however, not only measure correctly but, as their name implies, acquire the all desirable property of actually carburetting the air whatever its temperature and pressure!

Mr. LOVESEY: It has been his experience that the amount of heat taken away from the engine by the oil depends chiefly on the rate of circulation through the bearings. It is very little influenced by the total oil circulation through the whole system. The chief sources of heat input to the oil might be taken as (1) heat supplied due to mechanical friction, bearings, pistons, etc., (2) heat picked up from hot pistons, and (3) heat generated by the oil pump. With a large oil discharge from the bearings more heat is picked up from the hot pistons and in some cases this might amount to as much as 30 per cent. of the total heat given to the oil. Therefore it seems advisable to use the smallest oil discharge consistent with reliable lubrication. He had seen cases where a 50 per cent. reduction in the oil discharge reduced the heat given to the oil by 100 B.Th.U.'s per minute. When the oil circulation through the bearings was cut down, the outlet temperature of the oil was much higher and in that case it was an advantage, because the crankcase, which could be used efficiently as a cooler, is a much greater factor in dissipating heat. Greater advantage should be taken of crankcase cooling than at present by allowing adequate air circulation round the crankcase. Jacketing of the carburettor is apparently the only means of overcoming freezing, but whilst this danger would be overcome at full throttle openings, there would be the risk of boiling at small throttle openings and so cutting out the engine when such a light fuel as Swan spirit is used, because this boils very readily. He believed that the chief cause of de-tuning an engine and the non-agreement of test bench results with performance in the air when an engine is installed in a machine is due to defective balance systems.

Variation in consumption with different fuel feed pressures or heads should not occur with well designed float mechanism. The variation can only be due to alteration of the fuel level in the carburettor and this should not be more than 0.2 in. from the maximum to the minimum fuel head, which would not appreciably affect the consumption.

Mr. J. E. ELLOR: In view of the numerous auxiliaries such as petrol systems, water systems, cooling, ignition, carburettors, etc., they would be compelled to go over to compression-ignition engines in which the carburettors are replaced by fuel pumps. He did not think that the aircraft constructor had sufficient definite information with regard to specific values for radiators. There is, for instance, the question of the amount of heat that has to be dissipated with the falling off of altitude. This could be obtained on the test bench for ground level conditions, but more information is required with regard to altitude conditions, because in a naturally aspirated engine, the radiator is not designed to cool the engine when working full out on the ground as a state of balance is not obtained until a certain altitude is reached. What is that altitude and what is the order of radiator surface of frontal area to be applied? The time element comes in here because so much depends upon the time taken to climb to a certain altitude, and according to that time so the size of the radiator would vary. Carburation has always been a trying problem and always will be; no hard and fast rule can be laid down. A carburettor developed in connection with a particular engine will not necessarily give the same characteristics when applied to another engine of changed design. The author has gone to a great deal of trouble in explaining the effect of carburation at high altitude flying, but I would like to know the effect of low air temperature on distribution. It is known in car engines as well as aero engines that a lot of heat is taken out of the air in vapourising the fuel within the carburettor itself, so that the fuel is 60 per cent. vaporised under ground level conditions at normal air temperatures. The rest of the fuel is well atomised and the distribution ought to be

good, but at altitudes of 20,000 ft., where the air temperature is below −20°, the fuel will not vaporise, it will collect and settle down in the pipes and would not be distributed.

Mr. H. B. Taylor: The paper makes clear the vast difference between a motor car carburettor and an aeroplane carburettor. The section of the paper dealing with carburation could almost be described as a serious indictment of the aero engine carburettor. There would appear to be two main difficulties; the fluctuation of pressure at the air intake and the low temperature imposed on the fuel, both of which give rise to variations in mixture strength. The point he wished to make was that all such troubles would disappear when this bogey of mixture strength was eliminated. This would be done by pressing forward with the development of the compression-ignition engine in which the correct quantity of fuel will be directed to the precise place desired, namely, the cylinder, and will not be brought into contact with low temperature air and then asked to find its own way into the cylinder by a somewhat devious route in association with the correct quantity of air. In connection with supercharged engines there is a request for mixture coolers, but with a supercharged compression-ignition engine the last thing required is to cool the air; the hotter the better within limits.

Mr. A. F. Evans: Is any information available as to the relationship between the results obtained with a single-cylinder engine on the test bench and those obtained with a complete engine in the air under normal control by the pilot?

Reply to Discussion

Mr. Fedden's remarks on oil cleaners generally were very true. Cleaners were very necessary, and a considerable amount of work was being done both in this country and America with the aim of producing cleaners for cleaning both the oil and the air.

Apropos of the remarks of both Mr. Fedden and Mr. H. B. Taylor, regarding compression ignition engines, this type of engine may come in the immediate future, but the author's problems consisted in obtaining the best results and improving existing types. All engines had of course to go through a development stage, and he doubted whether the compression-ignition engine would achieve success as quickly as is often imagined. At present a single carburettor feeds perhaps nine or fourteen cylinders; injection pumps or metering devices will therefore need to be very reliable before fitting one to each cylinder.

Flight Lieutenant Bulman suggested that in racing, higher oil temperatures were obtained when flying at full throttle level than when climbing.

The author agreed, but racing is rather a special case, for in military flying, full throttle flying at low altitudes seldom occurs, the most general problem being climbing heavily loaded aircraft at low air speeds at full throttle to high altitudes. In order to reduce oil cooler size to a minimum, to permit clean aircraft design, the author is in favour of engines having low oil rates of circulation and high pressure.

With regard to the remarks of Mr. Hobson and Mr. Davey, it was a pity that the further modifications to the A.V.T. 70 carburettor had not been tested.

Mr. Davey suggested that oil heating of the carburettor was always preferable to water heating, but the reason for suggesting the retention of water heating was to simplify the piping on the engine as the carburettor and the induction pipes were usually close to the water jacket of the cylinders, but to bring oil up from the scavenge pump and then return it to the engine would complicate the engine lay out.

Mr. Davey had also made a point in regard to the high fuel consumption at low throttle openings. Carburettors at present were compromises. A choke

and jet setting is usually chosen which would give reasonable power and rapid acceleration in flight. If a pilot of a fast military machine made a false movement he must be able to open up the engine to full throttle instantaneously, but this is usually only obtained at the expense of economy. The case of the motor car or ordinary heavy air transport liner was very different, economy is the first consideration. He therefore believed that the lines on which development was proceeding were right, namely, to divorce power and consumption considerations from acceleration, and if rapid acceleration was to be secured without effecting economy and power, it would have to be done by means of some external device. Such a device, he believed, was already to be seen in advertisements of the technical journals. Messrs. Hobson submitted a similar device, *i.e.*, an injection pump, and this has been tried in flight at Farnborough, but although it was very promising it had not yet progressed sufficiently to enable him to report fully.

The variations in consumption with different heads were admittedly due to the fact that the float chambers of the old types of carburettors were poor and would not pass the present Air Ministry tests.

In reply to Mr. Elliott, the author stated that the carburettor of Fig. 12 was tuned on the test bed to give the best all round performance, a compromise being made between power, economy and acceleration.

Vibration does not affect modern float chambers because of their generous proportions, but the old carburettors were very susceptible to vibration and gave inconsistent consumption.

In reply to Mr. Burford, Mr. Penn said that the method suggested of cooling the crankcase was quite a practical proposition in the case of the motor car, but crankcase cooling introduced drag, which was a great objection from the point of view of aircraft designers.

The heating of carburettors by electrical means had been suggested, although he was not aware that it had actually been tried. Mr. Lovesay had provided the answer in his remarks, namely, there was a risk of boiling the fuel as the rate of flow in the carburettor was very low when the engine is idling.

In reply to Mr. Parnacott, the author stated he had no knowledge of any tests to determine the effect of vibration on fuel flow through pipes and that every precaution was taken to avoid air locks in fuel pipes.

Mr. Lovesey spoke of the oil system picking up much heat when using large rates of oil circulation. In this connection he wished he had given the table which he had cut out, more especially as several other speakers had also mentioned the point. It would, however, perhaps suffice to give one very low case. The Liberty engine, which had a rate circulation of about 25 gallons an hour, gave less trouble than most engines. Further, it had the excellent but unusual feature of measuring the pressure of the oil after it had passed through the engine instead of before. There was no doubt that with high rates of circulation of oil more heat was picked up, especially from a radial engine. There was one radial engine in which a very large circulation of oil was fed to the big-end by tubes in the crankshaft, the intention being to withdraw some of the heat from the big-ends. The rate of circulation was altered to something very much lower and the pressure put up. The amount of oil lost from the big-end and thrown in contact with the hot crankcase and underside of the pistons was materially reduced, with the result that the final temperature of the oil was considerably reduced.

Mr. Penn agreed with Mr. Ellor that quantitative tests of radiators were needed, but this depended on obtaining a suitable water flowmeter, a device not yet available for aircraft use.

It was quite true that on the ground only about 60 per cent. of the fuel was vaporised. At height very little vaporised. It was for this reason that one

would like to see hot air inhaled by the engine. In the supercharged radial engine the distribution at height of the wet mixture is materially assisted by the blower.

The author agreed with Mr. H. B. Taylor that the paper made an indictment of the carburettor as it stood at the present time, but he thought the time was not far off when the carburettor would be a better instrument than it had been for some years past.

Mr. Penn was unable to answer Mr. Evans question on the relationship between single-cylinder engines on the test bed and multi-cylinder engines in flight, as his activities did not extend to single-cylinder engines. It was not until engines had passed from the single-cylinder stage to the multi-cylinder stage and had successfully completed bench tests that they were passed on for flight tests.

The meeting closed with cordial votes of thanks to the author and the Chairman.

PROCEEDINGS

First Meeting, Second Half, 64th Session

In the Chair: THE PRESIDENT, COLONEL THE MASTER OF SEMPILL

A meeting of the Royal Aeronautical Society was held in the Lecture Theatre of the Royal Society of Arts, John Street, Adelphi, London, W.C.2, on Thursday, January 10th, 1929, when a paper on "The Streamline Aeroplane" was read by Professor B. Melvill Jones, A.F.C., M.A.

The PRESIDENT: When the history of aeronautics, covering the last ten years, comes to be written, the name of Professor Melvill Jones would figure in it very largely.

Professor B. M. Jones has been responsible for the initiation of several important researches under the direction of the Aeronautical Research Committee and has, in some instances, contributed not only papers but also carried out work as part of the general programme. The range of subjects on which he has touched has included control at low speeds, accidents, instruments, general control work and performance.

His most important work has probably been that directed by the Stability and Control Sub-Committee of the A.R.C. under his chairmanship. He drew attention to what was some six years a great obstacle to progress, viz., the inability of a pilot to retain adequate control of an aeroplane when about to land at any speed which was near the stalling speed. The panel (as it then was, but with the same name) co-ordinated a large programme of research on model, full-scale and theoretical lines, with the result that at the end of the work they understood what happened to an aeroplane when it stalled, having tried out various devices and pointed out where each failed and why. The success of the slot for control purposes is undoubtedly due in the greater part to the work of this panel, one of whose members was the first to draw attention to the use of a slot to fulfil the functions of that type of control which other work had shown to have a certainty of success. Professor Jones's own contribution included papers on the theoretical side and a certain number of flying experiments carried out in person. The same sub-committee has helped greatly in coming to an understanding of the features of a spinning aeroplane.

With a greater knowledge of the behaviour of a stalled aeroplane, it became possible to bring in a marked improvement in aeroplanes to reduce the danger of serious accident, which is so frequently a sequence to stalling in flight. Professor Jones did not hesitate to point out this matter with the result ultimately that many aeroplanes are now more adequately fitted with a suitable lateral control to retain balance when an accidental stall occurs. His flying experience has also been of appreciable value to the Accidents Sub-Committee in other connections to which a direct reference cannot here be made.

While the above work was in progress Professor Jones found time to carry out at Cambridge University a careful research on the use of sextants in flight. He has shown by a survey near Cambridge what accuracy is obtainable by the use of this instrument in an aeroplane for the determination of position.

During the past two years Professor Jones has been turning his attention to quantitative work on control. He has recently completed a first stage in a research showing by instrumental records exactly what happens to all the controls and to the aeroplane itself for certain specified movements made by the pilot. The flights were carried out largely by Professor Jones in an aeroplane specially loaned by the Air Ministry to the Cambridge University Air Squadron; the results of this pioneer research work provided a basis for similar experiments on other aeroplanes to be carried out at the R.A.E. and at Martlesham.

About a year ago Professor Jones first drew the attention of the A.R.C. to the great step forward that was possible in the improvement of aeroplane performance by comparing the resistances of an actual aeroplane with those theoretically possible. Much further evidence has been added since that date with the consequence that the A.R.C. initiated a series of researches on the interference of one part of an aeroplane on another. The first results of the interference research, carried out by a sub-committee under his chairmanship, are now to hand and have justified the prediction made by Professor Jones as to the large improvement that was possible.

Professor Jones has been an independent member of the A.R.C. since its appointment in May, 1920, and has assisted in a number of further matters to which that committee has paid attention since that date.

THE STREAMLINE AEROPLANE

BY

B. MELVILL JONES, A.F.C., M.A., F.R.Ae.S.

Ever since I first began to study Aeronautics I have been annoyed by the vast gap which has existed between the power actually expended on mechanical flight and the power ultimately necessary for flight in a correctly shaped aeroplane. Every year, during my summer holiday, this annoyance is aggravated by contemplating the effortless flight of the sea birds and the correlated phenomenon of the beauty and grace of their forms.

We all possess a more or less clear ideal of what an aeroplane should look like; a kind of albatross with one or two pairs of wings—depending on whether we live in Germany or Britain. In our more sanguine moments we even—like Alice and the cat—see the wings without the albatross. But progress towards this ideal, so far as the general purposes craft is concerned is, we must all admit, painfully slow. It has seemed to me that a contributory factor to the slowness of this evolution has been the lack of any generally understood and easily visualised estimate of what could be achieved were the difficulties in the way of realising the ideal form overcome. There is a natural tendency to decide on one day that the gain—say 20 per cent. on the total drag, or 7 per cent. on the speed—to be had by spending endless trouble on improving the undercarriage design, is not worth the trouble; on the next day to come to a similar conclusion about the drag of the engine cooling apparatus; on the next day about the wires, struts and minor excrescences; and on the next about the pilot's view; omitting to notice that if all the improvements were made at once the total gain would not be some insignificant percentage of the whole, but might reduce power consumption to a small fraction of its original value and so extend the range and usefulness of the aeroplane into realms which would otherwise be unattainable.

Considerations such as these led me some time ago to examine the possibility of devising simple formulæ for the power required by the ideal streamlined aeroplane, and this lecture is a result of that examination. The very simple

formula which I shall discuss is not precise—the present state of aerodynamical knowledge does not allow precision in this matter. It is not possible, for example, to say that lower power expenditures than my estimate might not occur. I am not so bold as to prophesy any limit to progress, but I think that I have made out a case that the power expended on the ideally streamlined aeroplane would not be greater than my estimate. Since this is of the order of one third the power expended on modern passenger carrying aeroplanes, the estimate should suffice, until such time as it more nearly represents actual performance; by which time we may hope that aerodynamic science will have progressed to a stage at which more accurate estimates can be made.

The formula at which I arrive is so simple and obvious that many will consider that I have wasted breath, ink and paper in putting it before you. That may be, but the difficulty lies in the choice of the quantities to be neglected and the argument here bristles with points where the scientist can pick holes. If you had had the job of putting a theoretically leaky argument such as this before a body of scientists, who are of necessity more concerned with detecting fundamental errors in reasoning than in devising engineering approximations, you would understand why I have gone to some trouble to expose, as precisely as I am able, the assumptions upon which the final conclusion is reached.

My lecture therefore deals with the possible reduction of aerodynamic head resistance, or drag, and hence of the power required for flight. At the outset it is perhaps worth noticing that such reductions will not greatly reduce the maximum power necessary for a given service, since maximum power is required for getting off from the aerodrome—an operation in which weight is the predominating factor. Reduction of drag will, however, enable an aeroplane of a given power loading, either to cruise at a higher speed or with a lower petrol consumption. This again will result in increased range or paying load, both factors of the first importance in aeronautical development.

Before the power required to overcome drag can be interpreted in terms of engine power, the airscrew efficiency must be taken into account. Efficiencies as high as 75 per cent. are practicable on present-day craft, and efficiencies higher than say 85 to 90 per cent. are unlikely to be achieved in the near future; hence the possible power economies to be obtained from further airscrew development—*e.g.*, through the use of the variable pitch screw—though by no means negligible, are unlikely to be very great. I shall not deal with the airscrew problem further, except in so far as the body-airscrew interference may influence my main argument.

Thanks to the combined efforts of Lanchester and Prandtl we can now isolate from the whole power that part—the power to overcome induced drag—which is expended in supporting the weight on a wing of finite span. This induced power, as it may be called, depends primarily on "*span loading*" that is weight/(span)^2. For a biplane of reasonable proportions the induced power per 1,000 lbs. weight can be expressed as

$$2.80^1\, w/\sigma V_m$$

where V_m is the speed, with one hundred miles per hour as the velocity unit.
σ is the ratio of the air density to that of standard air, and
w is the span loading in pounds per square foot.

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[1] The figure 2.80 is correct for an equal span biplane with rectangualr wings of aspect ratio 6 and gap/span ratio 0.13. The corresponding figure for a rectangular monoplane of aspect ratio 6 is 3.50. Both figures should be reduced by 5 per cent. for wings of elliptic plan, by $(A - 6)$ per cent. when the aspect ratio of rectangular wings is A, and by $100\ (G - 0.13)$ per cent. when the gap/span ratio is G. These refinements are unnecessary in the present paper.

The brake horse-power corresponding to the induced power is obtained by dividing by the airscrew efficiency. Assuming an airscrew efficiency of 75 per cent., an average span loading of say 2.2 lbs. per sq. ft., and making σ equal to unity, we get the following table for the b.h.p. required for the induced power in a normal biplane.

Speed m.p.h.	B.h.p. per 1000lbs. weight.		Ratio $\frac{\text{Total Power}}{\text{Induced Power}}$
	For Induced Drag.	Total normally supplied.	
90	9.1	35	25%
100	8.2	45	18%
120	6.8	75	9%
150	5·5	120	5%

Although the "induced power" is an important item in the power account at the lower cruising speeds, it is not the predominating factor at speeds above 90 m.p.h. Moreover, since it is clear from theory that no notable reduction in induced power is to be obtained without using much larger wing spans, I shall not in this lecture examine the problem of reducing it still further.

Since no great power reductions are to be obtained either by improving airscrew efficiency or by reducing "induced power," it remains to examine the possibility of reducing that part of the head resistance which arises merely because the aeroplane is being dragged through the air, without reference to the fact that it must support its own weight. Using terms in common use, this part of the whole drag is described as the sum of the wing profile drag and the parasitic drags. The power required to overcome it is seen in the table above to lie between 75 and 95 per cent. (according to speed) of the total power applied to the modern general purposes aeroplane. Any serious reduction in this item will therefore have an important influence on the total head resistance.

We all realise that the way to reduce this item in the power account is to attend very carefully to "*streamlining*." It is the main object of this lecture to examine how far this item can be reduced by perfect streamlining, and this incidentally will involve a preliminary explanation of what I mean by an ideally streamline aeroplane.

We recall first a proposition relating to that mythical and much abused substance, "the perfectly inviscid incompressible fluid." Theoretically when this hypothetical fluid streams steadily past a body, such as an aeroplane, the forces exerted on the surface of the body are everywhere normal to the surface; no tangential or skin friction forces can arise. The total reaction on the body, the resultant of all the pressures acting normally to the surface, is such that the "*drag*," or down-wind component, will be related to the "*lift*," or cross-wind component, according to the Prandtl theory of induced drag. In other words, Prandtl's induced drag is the sum of the down-wind components of all the normal pressures which would be set up by that steady flow of the hypothetical fluid which gives the required lift. It follows as a particular case of this proposition that the steady flow of the inviscid fluid which gives no lift also gives no drag.

In aeronautical nomenclature a "*streamline body*" is one about which the flow of a real fluid, such as air, approximates closely to a steady flow of the hypothetical inviscid fluid, except in a very thin layer called the "*boundary layer*," surrounding the exposed surfaces. In such a flow the pressures normal to the solid surfaces are very closely equal to the pressures of the corresponding flow of the hypothetical fluid, but the action of the real fluid in the thin boundary

layer causes additional tangential surface forces, or skin frictions, which are not present in the hypothetical flow.

An ideally streamline aeroplane may therefore be defined as one which:—

(*a*) Generates a flow identical, except in a very thin "boundary layer," with the flow of an inviscid fluid.

(*b*) Experiences a pressure distribution identical with that due theoretically to the inviscid fluid;

and therefore

(*c*) Experiences a drag which is the sum of the induced drag and the tangential or skin friction forces resolved in the down-wind direction.

Like all ideals, the ideally streamline aeroplane cannot exist; the boundary layer must have some thickness, so that the flow cannot be exactly the same as one in which there is no boundary layer. If the "*Reynolds' number*" is small, or in other words, if the effective viscosity of the fluid is high, the boundary layer becomes so thick as to make the above definition meaningless, but with the very high Reynolds' numbers, typical of aeronautical practice, the approximation for a good streamline body is very close.

It is, of course, well known that, unless bodies are carefully shaped, they do not necessarily generate streamline flow but shed streams of eddies from various parts of their surface. The generation of these eddies, which are continually being carried away in the air stream, requires the expenditure of power additional to that required to overcome induced drag and skin friction. The power absorbed by these eddies may be, and often is, many times greater than the sum of the powers absorbed by skin friction and induced drag. The drag of a real aeroplane therefore exceeds the sum of the induced drag and skin friction drag by an amount which is a measure of defective streamlining.

Having arrived at the conclusion that the drag of the ideally streamline aeroplane is the sum of the induced and skin friction drags, the next step is to estimate the magnitude of these drags. Methods of estimating the former are well known and an approximate formula is given on page 359. No corresponding theory yet exists for the estimation of skin friction on curved surfaces such as those of the wings and body of an aeroplane. We are therefore forced to adopt some empirical method of investigation. The simplest experiments on skin friction are those upon a thin flat plate edgewise to the wind, for here the resultant, in the down-wind direction, of the surface pressures is necessarily zero. The best experiments, of which I am aware, upon the drag of such a plate at high Reynolds' numbers were made at Göttingen some years ago. They lead to the formula

$$k_F = 0.019\, R^{-.15} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where k_F, the skin friction coefficient, stands for (skin friction)/$\rho V^2 E$ and R stands for $\rho Vl/\mu$ where

V stands for the relative velocity of air and plate.

E stands for the total exposed area of *both* sides of the plate.

l stands for the length of the plate in the wind direction.

ρ and μ are the density and viscosity of the air.

R is thus the Reynolds' number involving the dimension parallel to the wind direction.

The experimental range was approximately from $R = 2 \times 10^5$ to $R = 10^7$.

This expression (1) relates to conditions in which the "boundary layer" is turbulent over the greater part of the plate, that is to say, the air very close to the surface of the plate is eddying—as it were, rolling on the surface. Burgers, of Delft, has shown that near the front of the plate the boundary layer is not turbulent, the separate layers of fluid near the surface of the plate

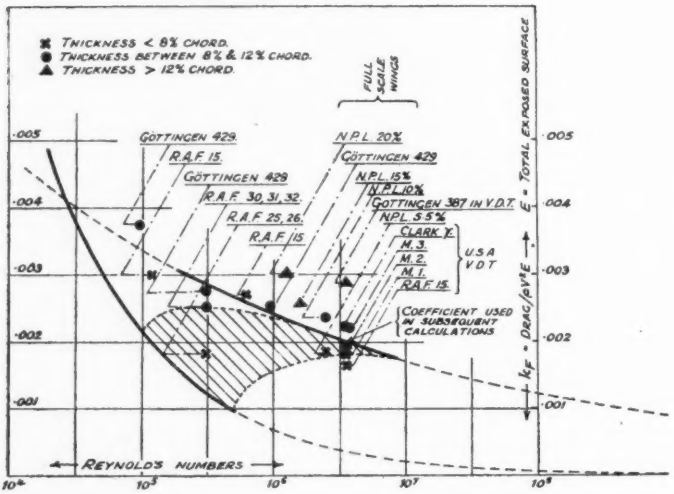
slip over each other smoothly. When the Reynolds' number is small this smooth region extends over the whole plate and the skin friction drag has, in these circumstances, been shown to agree closely with the theoretical expression

$$k_F = 0.66\, R^{-\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Thus at very low Reynolds' numbers the drag coefficient is as equation (2), but at high numbers as equation (1). In Figs. 1 and 2 I have plotted curves representing the expressions (1) and (2), above, on the assumption of smooth and turbulent boundary layers respectively. The upper curve in each figure represents expression (1) and the lower expression (2). At medium Reynolds' numbers, where the boundary layer is smooth over the front part of the plate and turbulent over the rear part, the average drag coefficient for the whole plate

FIG. 1.

COMPARISON BETWEEN PROFILE DRAG OF WELL-KNOWN WINGS AND THE SKIN-FRICTION ON A FLAT PLATE.



will lie between the upper and lower curves of Figs. 1 and 2; its exact value depending upon the distance from the leading edge at which the breakdown from laminar to turbulent flow occurs. The curve followed by the drag coefficient in this intermediate condition may be described as a "*transition*" curve, between the lower and upper curves of Figs 1 and 2. I have lightly dotted in roughly calculated transition curves,[2] on the assumption that the critical change from smooth to turbulent boundary layer occurs when the Reynolds' number formed from the distance of the breakdown point behind the leading edge has certain arbitrarily selected values, *e.g.*, 10^5 and 5×10^5.

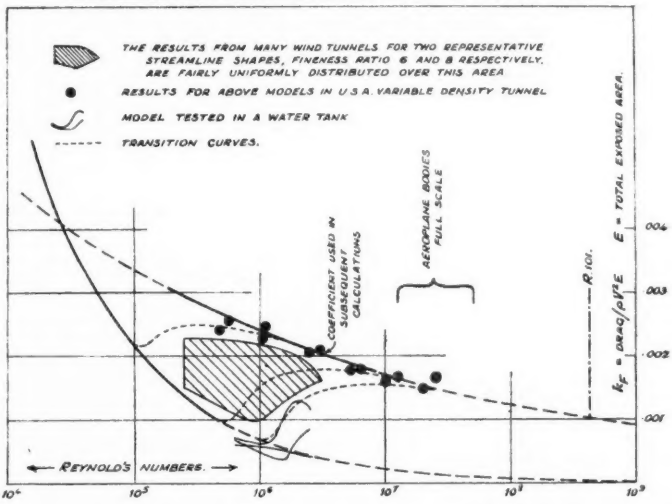
Burgers has found that for the flat plate the value of the Reynolds' number at this point of breakdown depends on the steadiness of the air in the tunnel before reaching the plate. When the flow was as steady as he could make it the change occurred when $R = 5 \times 10^5$. When the flow was deliberately made unsteady, by

[2] Transition curves of this nature have previously been calculated by Prandtl and shown to agree closely with direct measurements of drag. Ergebnisse der Aerodynamischen Versuchsanstalt zu göttingen. Vol. III., 1927. For assumptions made in calculating these curves see reply to discussion at end of paper.

bringing the wind tunnel honeycomb nearer to the plate or by placing wire gauze across the tunnel in front of the plate, the break up of the boundary layer occurred earlier, $R = 10^5$ approximately. Thus in the former case the drag coefficient of the flat plate should be expected to follow the lower curve until R has increased to 5×10^5 and then follow the transition curve which starts at this point, to the upper curve. In the latter case the change would take place along a transition curve which leaves the lower curve at $R = 10^5$. If it is assumed that the point of breakdown is uncertain between the above limits, the drag of the thin flat plate would be expected to follow the lower curve below $R = 10^5$ and the upper curve above $R = 5 \times 10^6$, but it might be anywhere within the shaded area when R lies between these two values. If the wind tunnel experiment were made on the plate between these two limits of R, the drag would be expected to vary erratically from tunnel to tunnel according to the turbulence of the air in the tunnel.

FIG. 2.

COMPARISON BETWEEN THE DRAG OF STREAMLINE SOLIDS OF REVOLUTION AND THE SKIN FRICTION ON A FLAT PLATE.



Our precise knowledge of the drags of very good streamline bodies other than the flat plate is confined at present almost entirely to the results of wind tunnel tests on solids of revolution, such as bare airship hulls, and on isolated wings. The former when moving axially exert no lift, and consequently their drag, if they conform to my definition of "*streamline*," should be entirely due to skin friction; the latter, when they exert lift should, if streamline, experience induced drag as well as skin friction drag. My first concern, when starting the investigation upon which this lecture is based, was to determine to what extent the drag of the better known streamline solids of revolution and the profile drag (total drag minus induced drag) of the better known wings, could be estimated from the known skin friction on a flat plate. To do this I have expressed the profile drags[3] of certain wing sections and the drags of certain streamline solids of revolution in the form

$$k_F = (\text{drag})/\rho V^2 E$$

[3] The points in this diagram refer to minimum profile drags, which however are, for all the wings included, close approximations to the actual profile drag at the incidence corresponding to cruising or top speed.

where E is the total exposed area.[4] These coefficients are plotted in Figs. 1 and 2, where they can be directly compared with the curves showing similar coefficients for a thin flat plate, as explained above. The Reynolds' number for these wings and streamline solids is of the form

$$R = \rho V l / \mu$$

where l is the maximum dimension of the body parallel to the wind—the "length" of the streamline solid and the "chord" of the wing.

Comparison in Figs. 1 and 2 between the experimental points and curves for the solid shapes and the curves for the flat plate at the same value of R, amounts to comparison between the drags of the solid shapes and of a thin flat plate of the same total exposed area, having a length parallel to the wind equal to the maximum length in this direction of the solid shape. I shall in future describe such a thin flat plate as the "*equivalent plate.*"

Figs. 1 and 2 are, I think, striking and significant. In Fig. 1 the points clearly tend to cluster around the upper curve, appropriate to the turbulent boundary layer, but one set of points, those for thin wing sections R.A.F. 25 and 26 at values of R in the neighbourhood of 3×10^5, show a tendency to fall towards the lower curve. We may summarise this diagram by the statements that:—

> When the thickness of the wing is less than 8 per cent. of the chord the profile drag is less than that of the equivalent plate. This includes nearly all wings used in Britain before some two or three years ago.
>
> Up to a thickness of 12.5 per cent. the profile drag is within some 10 per cent. of that of the equivalent plate. This includes the great majority of wings used in Britain at the present time.
>
> Thicker wings, up to 15 per cent. or 20 per cent., apparently have higher profile drag coefficients—between, say, 15 per cent. and 50 per cent. greater than the equivalent plate, according to shape and place of test.

Turn now to Fig. 2.[5] The bulk of the available information here results from extensive trials upon two models of bare airship hulls which were tested in a number of different wind tunnels in the various aerodynamic laboratories of the world, with the primary object of comparing the tunnels. The result was to show that wind tunnel experiments on very low drag models, such as these, are uncertain and depend to a great extent on the local conditions, such as turbulence, etc., in the tunnel. Speaking broadly, it may be said that the various wind tunnels gave experimental results for these models lying anywhere within the shaded area; you can take your choice as to which you are prepared to believe. There are clear signs, however, that as the Reynolds' number rises towards the extreme limit of atmospheric tunnels, the uncertainty becomes less and the tunnels tend to agree in placing the coefficient slightly below that of the equivalent plate. This tendency is somewhat faintly reflected in the shape of the shaded area.

The black circular dots relate to individual experimental results for these same two models in the American variable density wind tunnel. These points, which are carried to much higher Reynolds' numbers than were obtainable in atmospheric pressure wind tunnels, fit the upper or turbulent boundary layer curve with remarkable accuracy, even in the region of the lower Reynolds' numbers, where the majority of the atmospheric pressure tunnels give lower

[4] For simplicity in calculating I have assumed that for wings E equals twice the conventional wing area, and for solids of revolution E equals three-quarters of the area of the circumscribing cylinder. These approximations are good enough for practical purposes, provided that the ratio of length to diameter—the fineness ratio—of the solids of revolution is not less than 3:1.

[5] My thanks are due to the National Advisory Committee of the United States and to the British Aeronautical Research Committee for permission to include in Fig. 2 data which has not yet been published.

coefficients. The explanation of this is probably that the variable density tunnel employs a fine honeycomb, very close in front of the model, which should result, according to Burgers' experiments, in an exceptionally early break up of the boundary layer from laminar to turbulent flow. Thus under conditions in which, for a flat plate, the boundary layer would be turbulent over the majority of the plate, the drag of two different airship models is found to be closely equal over a very large range of Reynolds' numbers to that of the equivalent flat plate with turbulent layer.

The thin continuous lines near the bottom of the figure, in the neighbourhood of $R = 10^6$, relate to an experiment carried out on a different model in the experimental naval tank of the National Physical Laboratory. Here the fluid —water—was very still and the model was pushed from behind instead of being supported by wires attached at various points on its surface. External disturbances were thus reduced to a minimum. This treatment is seen to result in the coefficient falling very close to the lower curve appropriate to a flat plate with *laminar* layer. The experimental curves, however, begin suddenly to rise at Reynolds' numbers slightly greater than 10^6, in exactly the manner which would be expected if the layer at the rear part of the model were beginning to become turbulent at the higher Reynolds' numbers. That is to say, the curves rise approximately along the theoretical transition curves, which here have a slightly different shape from those in Fig. 1, owing to the tapered tails of the models. This same model was tested with the same method of support in one of the National Physical Laboratory 7 ft. wind tunnels, and produced a curve almost coincident with the lower of the skin friction curves in this figure. It was not, however, practicable in the wind tunnel to reach a Reynolds' number much above 10^6 and no signs of the rise in coefficient was observed. These experiments, both in air and water, were repeated with a thin string loop slipped on to the nose of the model, to form a slight obstruction which, from previous experiments of a similar nature, would be expected to cause the boundary layer behind it to become turbulent. With this addition the drag coefficients both in air and water increased some fivefold and approximated closely to that for the equivalent flat plate with turbulent boundary layer.[6] Thus, in circumstances where the boundary layer, from analogy with the flat plate, may be expected to remain laminar to a relatively high Reynolds' number, the drag of the model is very closely equal to that of the equivalent flat plate with *laminar* boundary, and the rise in coefficient, when it does occur, takes place in exactly the manner which would be expected on a flat plate with tapered rear. When, however, a slight change is made to the surface, which would be expected to break up the boundary layer over the greater part of the surface, the drag approximates closely to that of the equivalent flat plate with *turbulent* boundary layer.

I have examined the drags of other streamline solids of revolution in the same way as above, and I find that so long as the fineness ratio is not less than 4 the coefficients all fall between the upper and lower curves of Fig. 2.

Reviewing the evidence relating to streamline solids of revolution, it seems difficult to resist the rather surprising conclusion that their drag can be estimated with considerable accuracy from the drag of what I have called the equivalent flat plate. If the experiments recorded in Fig. 2 had been conducted upon thin flat plates of the same superficial area as the models and of the same length in the direction of motion as the overall length of the models, they could not have given results more in conformity with the known behaviour of the boundary layer on a flat plate, than the results actually recorded.

We do not yet know to what extent it may be possible to increase Reynolds' numbers whilst retaining a laminar boundary layer, but it seems probable that in the majority of instances the layer will be mainly turbulent before the Reynolds'

[6] These results are not shown in Fig. 2.

numbers appropriate to aeroplane bodies in flight—say, between 2 and 5 times 10^7—is reached.

For the purposes of the present paper it is sufficient to note that the upper or turbulent layer curve appears to give an upper limit to the drag coefficient of good streamline shapes, whether they be in the form of wings or solids of revolution, provided that the fineness ratios are not less than 8 in the case of wings and 4 in the case of solids of revolution.

The information displayed in Figs. 1 and 2 relates to simple shapes such as isolated wings and solids of revolution. There is as yet little information available relating to good streamline shapes in combinations, such as the wings and bodies of aeroplanes, or to the distortion and interference to which a shape can be subjected without becoming unstreamline. Such information as is available suggests that combinations, or minor distortions, can be made without appreciable increase of drag, provided we know how to make them. It is therefore reasonable to suppose, after examining Figs. 1 and 2, that complex bodies such as aeroplanes could, given sufficient knowledge and structural ability, be made to have a drag no greater than the sum of the induced drag and the drag of the equivalent flat plate.

If the drag of the streamline aeroplane is simply the sum of the induced and skin friction drags, the power required to tow it would be obtained by multiplying these drags by the forward velocity V. Let I and F be the powers required to overcome induced and skin friction drags respectively. If the streamline aeroplane is towed by an airscrew of efficiency η, effectively isolated from it so that there is no interference, then the b.h.p. required to drive the screw will be

$$(I+F)/\eta$$

How are we to regard interference between screw and body, when such exists? Interference will alter torque and thrust reactions between screw and engine. The change in torque may call for a redesign of the screw to make it balance engine torque at suitable engine revolutions. The change of thrust is of no interest, except in relation to the size of thrust bearing required, for it may be largely neutralised by change of body drag. Neither change is of direct interest from the present point of view, which is to examine any possible changes in b.h.p. due to interference, assuming always that *the screw is properly designed for the engine*. With this assumption it is not difficult to show that, to a first approximation, the b.h.p. required to propel the *streamline* aeroplane should not be seriously influenced by interference, provided that the interference does not cause the flow to cease being streamline.

We arrive at this conclusion by considering the disturbances left in the air behind the streamline aeroplane. They may be divided into:—

1. The airscrew slipstream.
2. The induced vortices.
3. The small scale turbulence, and possibly temperature rise, due to skin friction on the exposed parts of the airscrew blades and on the aeroplane.

Since, by hypothesis, the flow around the streamline aeroplane is substantially that of an inviscid fluid, except in the very thin boundary layer where the skin friction is applied, the *whole energy* expended—the b.h.p.—is included in the above three items.

The energy in the slipstream is mainly due to the axial velocity of the stream; it is dependent upon the effective diameter of the stream and the net thrust of the screw and body behind it. If, as we may reasonably suppose, these are unaltered by the interference, item (1) will remain substantially unaltered.[7]

[7] This argument is not true if the rotary velocity of the slipstream is taken into account, but the energy in the rotary motion is generally very small.

The energy I in the induced vortices will not be altered by interference, unless the interference appreciably alters the lift distribution across the wing span. If considered necessary, the alteration could be estimated and I taken to apply to the induced power associated with the actual lift distribution in the presence of interference.

The energy expended on skin friction on the airscrew blades will not be seriously affected by any reasonable interference.

The energy F expended on skin friction on the aeroplane will be affected only on those surfaces which are exposed to the slipstream. This effect will always be small; at high speeds, where F is an important item, the slipstream velocity is low, whilst when climbing F is but a small part of the power expenditure. To a first order, therefore, this item in the power account will also be unaffected by interference, but if higher accuracy is required F must be taken to relate to the estimated skin friction with allowance for increased velocity over surfaces exposed to the slipstream.

I do not maintain that the above arguments are precise, but if they are examined carefully, with due regard to the quantities involved in practicable aircraft, I believe that they will be found sufficient.

We are now in a position to state the main conclusion of the lecture, which is as follows:—

The brake horse-power required to propel the streamline aeroplane horizontally may be estimated as

$$(F+I)/\eta$$

where η is the efficiency to be expected from an *isolated* screw performing the service required.[8]

F is the power expended in skin friction, which may for the present be taken to be that required to propel the equivalent flat plate.

I is the induced power.

As a first approximation F and I may be estimated for the aeroplane isolated from the screw. As a second approximation F can be altered to allow approximately for the increased velocity over surfaces exposed to the slipstream and I to allow for alterations in lift distribution across the wing span due to the slipstream. In the estimates which follow the first approximation is sufficient for my purpose and I have not proceeded beyond it.

When the streamline aeroplane is climbing, the above expression for brake horse-power must include another term C, equal to the product of weight and rate of climb. Thus

$$\text{b.h.p.} = (F+I+C)\,\eta$$

It remains to give numerical expression to the symbols I, F, C and η.

The estimation of I presents no difficulty. A simple formula $I = 2.80\,\varpi/\sigma V_m$ horse-power per thousand pounds weight with span loading ϖ in pounds per sq. ft. and V_m measured in hundred-mile-per-hour units has already been given. This formula applies strictly to a biplane of normal gap and aspect ratio and of rectangular plan form, but is easily modified (see footnote on p. 4) to apply to a monoplane or biplane of any desired gap or aspect ratio.

The skin friction power F depends upon the skin friction coefficient

$$k_F = (\text{skin friction drag})/\rho V^2 E$$

and before F is estimated some value for k_F must be adopted. The question arises as to what value is to be given to k_F. If it is to be taken from the value

[8] As an example of what is meant by a screw performing the service required: a screw of fixed pitch which has to give a compromise between the requirements of take-off, climbing and cruising speed will not have so high an efficiency as would be obtainable were it only required for cruising. η might thus be 75 per cent. cruising and 70 per cent. climbing, whereas were a screw of variable pitch contemplated η might be over 80 per cent. cruising.

for the equivalent flat plate, *i.e.*, from the upper curve of Figs. 1 or 2, its value will depend on the Reynolds' number, and this raises the further question as to how the Reynolds' number for the aeroplane is to be estimated. The Reynolds' numbers of full scale aeroplane wings, based on the chord as a measure of size, lie between 4×10^6 and 10^7 and the corresponding value of k_F for a thin plate (see Fig. 1) lies between 0.0020 and 0.0017. The Reynolds' number of the bodies of full scale aeroplanes, based on their length as a measure of size, lie between 2×10^7 and 5×10^7, and the corresponding value of k_F (see Fig. 2) lies between 0.00155 and 0.00135. The logical proceeding would be to estimate the wing skin friction drag from Reynolds' number appropriate to the wings and the body drag from the Reynolds' number appropriate to the body. But for simplicity and so as to be on the safe side, I propose, for my present purpose, to take an over-all figure for k_F of 0.0020. This should leave us a little in hand to allow us to use moderately thick wing sections—say, thickness up to 12 per cent. of the chord—in our ideally streamline aeroplane.

Using this figure we have

$$F = 0.002\,\rho V^3 E \text{ foot pounds (using any consistent units).}$$
$$= (27\,\sigma V_m^3 E)/W \text{ horse-power per 1000 lbs. weight.}$$

where V_m is in hundreds of miles per hour units, E is the exposed surface in square feet, and W is the weight in pounds.

Finally, the climb power C, per 1,000 lbs. weight, is easily shown to be given by $C = 0.030\,V_c$ where V_c is the rate of climb in feet per minute.

These three simple expressions for I, F and C, allow an approximate and probably conservative estimate to be made of the power that would be required by an aeroplane in any given circumstance, provided that the flow around it were everywhere streamline and no unnecessary eddies were generated. The difference between the actual power expended and the power so estimated must be regarded as being expended in the generation of unnecessary eddies which might be avoided by more careful attention to external form.

I have thought it of interest to prepare a few curves showing the performance, in level speed and climb, of a streamline biplane in air of standard density near ground level. For this purpose I have taken $E = 3.2 \times S$ where S is the conventional wing area, so that $F = 86\,\sigma V_m^3/\omega$ where ω is the wing loading in pounds per sq. ft.

I arrived at this value 3.2 by computing E roughly for a number of well-known aeroplanes and finding that E/S ranged between 3.0 and 3.5 with 3.2 as an average value. In drawing the curves I have taken σ to be unity and η, the screw efficiency, to be 75 per cent. for level flight and 70 per cent. climbing.

Fig. 3 shows four curves giving b.h.p. per 1,000 lbs. weight, for level flight at various speeds for, the following conditions:—

wing loading (ω) 10 and 15 lbs. per sq. ft.
span loading (m) 2 and $2\frac{1}{2}$ lbs. per sq. ft.

The individual points, with the names of well-known aeroplanes written against them, refer to the b.h.p. at top speed[9] as given in the 1927 edition of Jane's "All the World's Aircraft."[10] The small circles relate to aeroplanes for which ω is approximately 10 and the squares to those for which ω is in the neighbourhood of 15.

[9] I am aware that cruising speed is of more general interest than top speed, but I have used the top speed in computing these points because of the difficulty of estimating engine power at cruising speed. The broad conclusions would be much the same in either case.

[10] I took the figures from Jane to avoid argument. Being, as I suppose, makers' own figures, they are unlikely to be pessimistic as regards performance. Some of the more interesting aeroplanes are omitted because performance figures are not given in Jane and I was loth to introduce a new variable by writing to the firms concerned and getting up-to-date figures for some types as compared with the 1927 figures for others.

The vertical distance between the points and the appropriate curve represents the power being expended on what, from the present point of view, must be described as unnecessary turbulence. Horizontal distances from the curves represent the increased velocity which might presumably be obtained for the same power expenditure in the absence of unnecessary turbulence.

Apparently large commercial aeroplanes, such as the Argossy, the W.10 and the Hercules, would, were they ideally streamline, either fly at the present top speed for one-third the present power, or alternatively travel some sixty miles per hour faster for the same power. Two-thirds of the power used by these aeroplanes is being expended on the generation of turbulence which, from the aerodynamic viewpoint, is unnecessary.

FIG. 3.

COMPARISON BETWEEN REAL AND STREAMLINE AEROPLANE.
FIGURES FROM 1927 JANE. TOP SPEED.

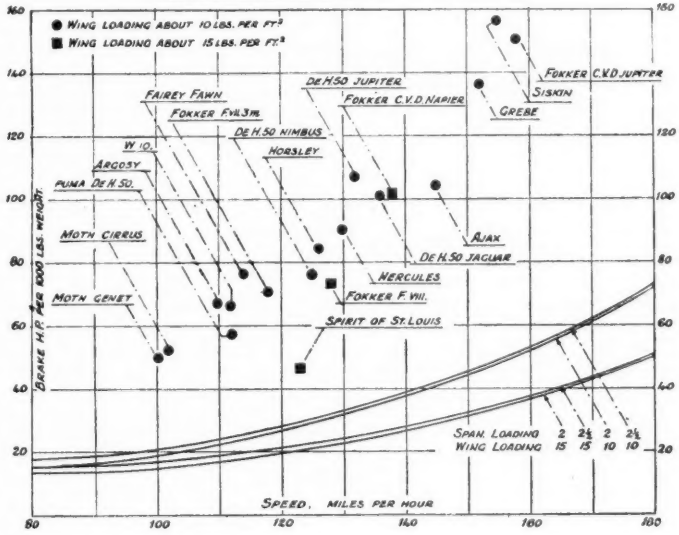


Fig. 4 is constructed on the same lines as Fig. 3 except that the abscissæ represent rate of climb and the three continuous lines show the expenditure of horse-power required for a streamline aeroplane, divided up into the three parts: weight-lifting power, induced power and skin friction power. Here the span loading is 2.2 lbs. per sq. ft., which is a good average figure for modern aeroplanes, whilst the wing loading is taken as 10. The airscrew efficiency is taken to be 70 per cent.

Though improvements in power consumption for climbing are not so great as for top speed, they are still appreciable. The very high ratio between the power apparently expended in turbulence in some cases and the power lost in skin friction, suggests very high turbulence losses, due prboably to severe interference between the slipstream, the wing and the body in these circumstances. Possibly the assumed airscrew efficiency of 70 per cent. is too high for climbing conditions on modern commercial machines, and this may account for some of the heavy losses indicated in the diagram.

Recapitulation

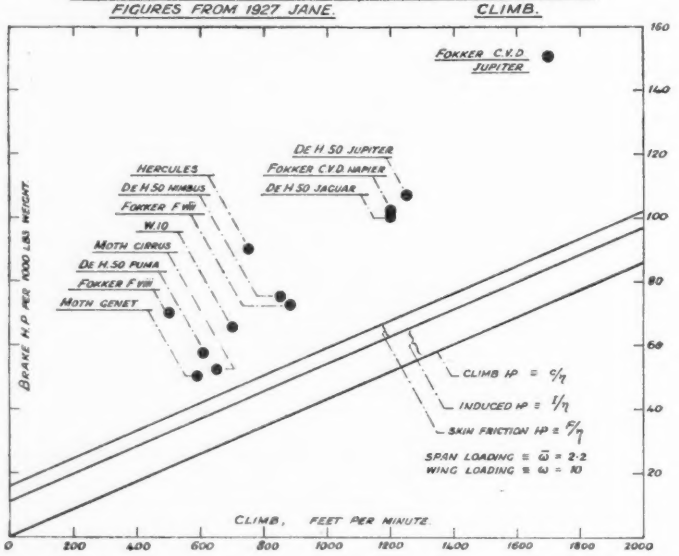
The lecture consists of a search for a simple and reasonably sound way of estimating the power which would be required to drive a perfectly streamline aeroplane. The formula finally reached is very simple and easily visualised; the difficulty lies in defending the approximations by which it is reached.

There are two main steps in the argument: the first is that the power required to tow the ideally streamline aeroplane should be merely the sum of the induced and skin friction powers. This idea suggests the use of a drag coefficient, based on the total exposed area, to estimate that part of the power which is not absorbed in the easily calculable induced drag and in climbing.

A comparison of such drag coefficients for a series of simple streamline shapes, with the skin friction coefficient of a thin flat plate, with turbulent flow in the boundary layer, suggests that the latter provides a convenient and safe estimate for the drag coefficient of any good streamline body.

FIG. 4.

COMPARISON BETWEEN REAL AND STREAMLINE AEROPLANE.
FIGURES FROM 1927 JANE. CLIMB.



Finally, a figure of 0.002 is shown to give a conservative estimate of the coefficient for wings and bodies at the Reynolds' numbers of full scale flight.

The second step in the argument is that in the streamline aeroplane, interference between the airscrew and the aeroplane, though it may affect the design of the screw suitable to a given engine, should not greatly affect the over-all power absorbed; hence, when estimating the brake horse-power required for the streamline aeroplane, the screw efficiency may be assumed to be that of an isolated screw.

These two conclusions, if accepted, enable the power required by the streamline aeroplane to be very easily calculated. It is submitted that this should always be done in order to keep before the designer and other people concerned the price that is being paid for defective streamlining.

It is not suggested that it is easy to design a streamline aeroplane which will be also a practicable machine, but the immense saving in power, and therefore in fuel consumption, which would apparently follow such a step, forces me to the belief that design will evolve steadily in this direction and that the ultimate aeroplane will be as well streamlined on the whole of its external surfaces as, say, the bottom of a racing yacht or the externals of an albatross. I am fortified in this belief by surveying the animal kingdom. Those birds and fishes which depend on speed for their existence have long since solved the problem. The compromise with structural difficulty was no doubt as difficult for them as for us, but it has ended in the complete triumph of the external form. We can only hope that it will not take us so long to reach this point as, if we are to believe the comparative anatomists, it took them.

Discussion

The President: Professor Melvill Jones had drawn the attention of the Aeronautical Research Committee a year or so ago to the matters dealt with in the paper; as a result, experiments were carried out, which had in substance proved his contentions. He had set up a valuable standard, and when one considered how far present-day aircraft, both military and civil, were below that new standard, one could hardly cavil about its supposed inaccuracies. He had stated in the paper that his conclusions were based upon leaky theoretical arguments, but one felt that when the resistances of present-day aircraft had been reduced by about 50 per cent. it would be time enough to consider those leaky theoretical arguments with a view to their revision.

Major F. M. Green: He thanked Professor Melvill Jones for having set up not merely an ideal, but an ideal which could be expressed in figures. It meant much more to a designer to be able to say that his aeroplane represented a definite advance towards a definite ideal, than to be able to say merely that it was a little better than its predecessor, because in the latter instance he did not know how much farther he had to go. Designers were now aware that a great deal of progress must be made, and the paper would stimulate them to make better aeroplanes. The influence of figures was very great, and designers would be able to express the efficiency of their aeroplanes as a percentage of Professor Melvill Jones' ideal, instead of saying merely that a particular aeroplane was fairly well streamlined or was fairly efficient. With regard to the overall figure of 0.002 for the skin friction drag of planes, he said that was half what was at one time called the minimum profile drag (0.004). The figure would vary, however, with different angles, and with greater angles the figure might be greater. With regard to turbulence, that in the air occurred on a very much greater scale than in a wind tunnel, and he asked whether the results obtained with an aeroplane on a day when there was considerable turbulence would be appreciably different from those obtained on a calm day.

Mr. R. McKinnon Wood, referring to the use of the wind tunnel, said that for many years it had been thought that the ideal condition of test was that the turbulence of the air in the tunnel should be the minimum attainable, but in the last year or so it had dawned on some investigators—it had dawned on him a few months ago—that possibly the wind tunnel which was regarded as the worst was really the best; because, if there were sufficient turbulence in the air the results obtained would be as indicated by the upper curve exhibited by Professor Melvill Jones, and if one extrapolated along such a curve one should make a fairly good prediction of the full-scale value. If the results lay below that curve—somewhere in the indefinite area—one could not say in the least what the full-scale value might be. He hoped that the paper would be of great service by focussing the attention of designers of aircraft more on aerodynamic considerations. He would not reproach designers for giving, as he considered

they did, too much attention to weight, to mechanical, structural and other aspects of their work and too little to the aerodynamic aspect. The reason could be found very simply; one could determine the weight of a machine easily and precisely, but it was more difficult to determine drag and to determine what improvements were effected by small alterations. A question which was continually before those engaged in full-scale tests was what improvement was effected by replacing, say, a radiator by a different radiator. The gain in speed effected thereby was probably half a mile or one mile per hour, but the man who was making the tests knew that variations in engine power, up and down currents, and other factors might affect speed to a greater extent than would the changing of the radiator. He believed that this work of Professor Melvill Jones, in that it showed how much better a performance we could look forward to, would have the effect of focussing the attention of designers more and more on the problem of reducing drag.

Mr. A. FAGE: He would like to mention two series of experiments with which he had been connected, and which had a direct bearing on Professor Jones' paper. It was well known that on a large circular cylinder there was a certain range of Reynolds' number (VD/ν) over which the character of the flow underwent a marked change, and which was accompanied by a large drop in the drag coefficient. He had had occasion to explore the boundary layers for these flows and to determine the breakaway points. Denoting the circular distance from the stagnation point to the breakaway point by "l," and the velocity just outside the boundary layer at the breakaway point by "V," he had found that the range of Reynolds' number (Vl/ν), over which the marked change of flow occurred, was from 1.1×10^5 to 4.4×10^5. This was exceedingly interesting, because, as mentioned by Professor Jones, Professor Burgers had found that the critical range of (Vl/ν) for a flat plate (l is the distance from the leading edge) over which the flow along a flat plate could break down was about the same order, namely, 10^5 to 5.0×10^5. There was, therefore, from this point of view, a resemblance between the flows in the boundary layers along a flat plate and around a cylinder, and we should expect intermediate bodies such as airship forms and aerofoils to exhibit similar tendencies; Professor Jones had shown that they did. Another point of interest arose from some experiments made to measure the minimum drags (at zero incidence) of a family of symmetrical Joukowski sections of different thickness. If one plotted drag coefficient against thickness one was able to predict the drag coefficient for a Joukowski aerofoil of zero thickness (*i.e.*, a flat plate). The values of Reynolds' number at which the predictions were made were in the critical region, and as would be expected the predicted values of drag coefficient lay between those for laminar and turbulent flows along a flat plate; further, these predicted values agreed fairly closely with Geber's values for a flat plate at the same Reynolds' number. These results gave additional support to Professor Jones' conclusions. A great merit of the paper was that attention was focussed on the large amount of power frittered away in driving modern aeroplanes, and the paper should prove a great stimulus to all workers in aeronautics.

Mr. C. C. WALKER: Those concerned with aviation were particularly interested to know what chances there were of recovering some of the lost power, and how much of the loss was inevitable. When Professor Jones had first published some of his results, he (Mr. Walker) had made calculations in respect of a number of aeroplanes, taking, instead of approximations, the actual "wetted" surface of each aeroplane and the available h.p. (after multiplying the b.h.p. by the propeller efficiency) and had found that an ordinary commercial aeroplane did attain from 60 to 67 per cent. of the streamline speed. Both the best and the worst examples among his own company's machines happened to be monoplanes. In a little racing machine which did 197 miles per hour with 133 h.p.,

a little over 80 per cent. of the streamline speed was attained. That meant that what had to be recovered amounted to the equivalent of three-quarters of a square foot of flat plate presented normally to the wind, and that had to include the undercarriage, all the bracing, the cooling of the engine, the cockpit, and the effects of the slipstream on the body and wings. The machine must have an undercarriage, and at present it must have some form of cockpit; cooling resistance might be reduced somewhat, but there seemed to be very little margin for recovery in a machine of that sort. In the case of a commercial aeroplane, which must have all sorts of other excrescences, it was difficult to foresee how far one could go towards eliminating the resistance. Such a machine had a radial engine sometimes on the front of the body. the cabin had to be ventilated by means of structures like ships' ventilators, sometimes there was a starting engine mounted outside the fuselage, and it did not matter very much what one did in the way of fairing after that. It would be interesting to know what Professor Jones thought about the amount that should be saved. (Laughter.) Referring to the problem of induced drag h.p., he said that Lanchester, in 1914, had shown how to work it out, and from his book, which was published in 1908, one found that he had put it forward to the Physical Society in 1894. At that time he had called it "aerodynamic drag," but it appeared to be the same as "induced drag." He (Mr. Walker) did not know whether or not Prandtl had worked it out earlier than that, but the point was that if "aerodynamic drag" were the same as "induced drag," and nothing more than a change of name was involved, it was rather a pity that Lanchester should be deprived of the credit due to him.

(Communicated): There is an aspect of Professor Jones' criterion of performance which is not free from objection. Presumably, by encasing the various necessary excrescences in large streamline boxes, or by so increasing the fuselage in size as to embrace what otherwise would not be embraced, credit could be taken for the extra surface, and a high streamline efficiency shown.

It could then quite easily happen that of two aeroplanes doing the same job, the slower would show a better figure of merit on this basis than the faster.

In one of Professor Jones' earlier papers, he placed on the credit side the area of the floats of a seaplane—as no credit can be taken for the wheels of the corresponding land machine—the situation mentioned above may be said to have already arisen.

The fact remains that the great virtue of this method is its absolute nature.

For purely comparative purposes, it is perhaps preferable to give the highest figure of merit to that aeroplane which carries a given paying load per h.p. fastest in relation to its landing speed or to use some similar basis. The "streamline efficiency" will then show to what extent this machine falls short of the ideal, but to aim directly and only at this in design would not necessarily produce the best aeroplane.

Mr. W. L. COWLEY: It would appear from his remarks that the wind tunnels had given us fictitious results in the past, but he would like to hear the lecturer explain the fact that, in many cases, excellent agreement had been obtained between model results and full-scale. As far as his personal knowledge went, designers had the greatest faith in wind tunnel work. The host of results upon various wing, strut, body and float forms tested in the past as well as on complete models, airscrews, controls, stability, etc., were made use of daily and no marked discrepancies with full-scale, except in occasional cases such as R.A.F. 19, had been reported. It would be strange if these agreements were purely accidental and still more so if they were but imaginary, and that this fact had escaped the great army of workers that had been engaged upon the subject.

Just before attending the lecture he calculated the drag coefficients of an aerofoil, a body and a float that were tested some time ago in connection with a

certain machine. The points fell almost exactly upon the turbulent boundary layer curve given by Professor Jones. Now Professor Jones assumed that the drag curves for the airship forms ultimately merged into this turbulent boundary layer curve. If it be assumed that the drag curves for the parts mentioned should also follow this curve, the drag coefficient for the full-scale machine should have been approximately 0.0014 instead of about 0.0022 as the model scale effect curves indicated. The full-scale result appeared to be more consistent with the latter than with the former result. It may be argued that the resistance measured were only partially skin friction. If that was so one would expect the difference between 0.0022 and 0.0014 to be form resistance and the curves of drag coefficients of the models against log Vl/ν to follow a transition curve rising up rapidly to a turbulent boundary layer curve parallel to the one given, but having the ordinates greater by an amount equal to this difference. In the tunnel tests mentioned, the variation of drag coefficient with log Vl/ν however, was very slight. During recent years tests have been made at the National Physical Laboratory upon several models of complete machines, ranging from single-seater single-engined machines to large three-engine machines. Parts were tested alone and in combination and the whole work has been carried out in direct touch with the designers of the machines concerned. In no case has there been the slightest cause to suspect the results. It should be noted that the scale effect was considered so small that the results at the highest values of Vl/ν in the tunnel tests could almost be applied directly to full-scale without correction. Further, the parts were of fairly fine form and the minimum drag coefficients were low. In one machine the model results indicated a higher maximum lift than was expected, but the performance of the machine was found to agree remarkably well with all the model test results.

Reverting to the skin friction curves given by Professor Jones, it would be noticed that the phenomenon of double flow and the transitional region was similar to flow in pipes, but in the latter it was usual to make in the expression log Vl/ν equal to the diameter of the pipe and not to a down wind dimension. Now it was implied by Professor Jones that surface area at the rear of the body was affected differently from that near the front on account of the accumulated disturbance produced in the boundary layer by the action of the surface coming before; and the average drag per unit area of the surface of a good streamline form depends only upon the length of the body when the speed, density and viscosity were constant. Now the resistance of a parallel pipe was all skin friction and its drag coefficient appeared to be of the same order as that for the flat plate. One would expect, therefore, similar conditions to prevail. Thus, if for the flat plate, the boundary layer became turbulent at a distance l from the leading edge, then it would be expected that turbulence commenced in a pipe at the same distance from the entry. In other words, turbulence in a pipe commenced at a certain distance from the end irrespective of the diameter, so that, for two pipes a and b using the same fluid, the distances and speeds were related by

$$V_a l_a = V_b l_b.$$

The dimensional theory, however, demanded that, if the conditions were similar, a breakdown in flow would occur at corresponding points when

$$V_a D_a = V_b D_b$$

where D is diameter. In other words, the diameter is all important. It followed, therefore, that the main body of the fluid affected the resistance as well as the surface fluid.

The apparent agreement between the experimental pressure distribution round a body and the pressure distribution calculated from the inviscid fluid theory gave no support to the contention that the integral effect on drag of the flow in the main part of the fluid was negligible. After all the integral of the dis-

crepancies between the pressure distributions, when resolved, was equal to the whole of the drag. One could only conclude that the inviscid fluid theory, in its present form, might be good enough for calculating certain effects, but was useless for any work involving profile drag.

In conclusion, he said that although he regarded the lecture as very interesting and instructive, he thought it was a little dangerous to act upon any deduction made from it at the present stage of our knowledge. It did not justify as yet any loss of faith in wind tunnel work or any change in wind tunnel technique, although he agreed that further research on the lines given by the lecturer might be necessary.

Mr. PYE: Referring to Professor Jones' curves showing the b.h.p. per 1,000 lbs. weight in respect of certain machines for level flight at various speeds, he suggested that one or two spots representing the performance of machines which competed in the Schneider Trophy contest might be added, because the drag had been very materially reduced in the case of those machines. If such information were given, one would be able to make extraordinarily interesting comparisons, because the drag of those machines must be very much nearer to Professor Jones' theoretical limit than it was for any of the machines he had shown.

Dr. DOUGLAS: No doubt in cases where the full-scale flow was turbulent increase of turbulence produced a similar effect to that obtained by increasing Reynolds' number, and a better idea of full-scale value would be obtained if models were tested in a very turbulent tunnel, as suggested by Mr. McKinnon Wood. It was quite possible, however, that on objects, such as struts, the full-scale flow might actually be laminar.

Professor Jones had shown that existing aeroplanes have very much more drag than they need have. Machines were defective both in form and in surface. The latter defect was particularly noticeable on the Schneider Trophy winner displayed in London, and he had been shocked to notice the rivet heads and other excrescences on a machine designed to have the lowest possible drag. The components of a machine of this type have drags little greater than that given by thin plates of similar surface area, and when this standard had been reached it must pay to keep the surface clean. He hoped that Professor Jones' remarks would lead to a better appreciation of the need for reducing drag.

Mr. IRVING: With regard to the point raised by Dr. Douglas, Professor Jones, in arriving at his overall figure for the full-scale drag coefficient, had assumed that one travelled along the upper curve. It was just conceivable, however, that with certain shapes one might possibly keep to the lower curve. The departures from the lower curve had in most cases occurred in wind tunnel tests in which the general flow was turbulent, although in one case, in which tests were made by towing a model in still water, there was also a departure from the lower curve in the direction of the upper curve. Mr. Irving also referred to some tests carried out at the National Physical Laboratory in 1919, in which Mr. Ower and he had measured the skin friction of flat plates of various thicknesses on the balance in the wind tunnel, and extrapolated from different thicknesses down to no thickness. The values of the skin friction coefficient obtained for different sizes of plate at different wind speeds were plotted against VL; for the lower values of VL the points came actually on the lower curve, but as VL increased there was a gradual departure, and at the higher values of VL the curve crossed the region between Professor Jones' two curves and almost touched the upper curve. He mentioned this fact because he understood Professor Jones' lower curve was based on measurements of flow near flat planes and not on overall measurements of the skin friction on a balance.

Mr. CAPON: On a previous occasion when Professor Jones had lectured to the Society, he had uttered the dictum that no research could be considered to

be completed until it had been reduced to a rule of thumb. One might add that no research should be begun until there was a definite formula to work to, and the great value of Professor Jones' work was that he did provide such a formula. It had been known for a long time that great savings in drag could be effected; probably the reason why so little had been done was that there were a great many small contributions, and nobody felt really convinced about any of them. For example, it had been known for several years that a saving in drag on models could be effected by cowling the cylinders of radial engines. Why were radial engines not cowled? Probably because designers quite reasonably thought that there might be a scale effect on drag or cooling, or both. Again, we really did not know how much the sum of these contributions would amount to. Professor Jones had given a figure of merit to indicate what could be expected, and although there might be some quibbles as to what exactly that figure amounted to—questions such as airscrew interference, and so on, were involved—it was nevertheless a sufficiently definite figure. Professor Jones was to be congratulated, not only upon having produced it, but on having met so successfully, as it appeared, the objections raised by the scientists based on the uncertainty as to the skin friction coefficient to be adopted. His investigations in this direction had apparently led to results of great interest in other connections.

Mr. Scott Hall: It seemed to him that Professor Jones' assumptions with regard to the working of the screw in close proximity to the body were of a very grave nature, for in effect he had assumed that the loss due to increased drag of the body in the slipstream behind the screw was balanced entirely by the extra efficiency with which that screw worked, due to the body behind it. He would have liked to have heard more from Professor Jones on that matter. With regard to the curves showing the performance of various aircraft, he said that the point relating to the "Spirit of St. Louis" was situated a considerable distance from those relating to other machines. The "Spirit of St .Louis" was the only machine dealt with which had no open cockpit, nor openings of any kind, and that fact seemed to indicate that the openings on a streamline body were of very serious practical importance, more so even than had been hitherto supposed.

Mr. Bramson: He could not resist the temptation to point out one analogy; in all probability Professor Jones' treatment of the subject of the streamline aeroplane would be regarded as holding the same position in relation to aerodynamics as, for instance, the Carnot cycle held in relation to thermo-dynamics. Professor Jones had put forward an ideal beyond which, without the discovery of new principles, one could not hope to get, and he had given designers a standard by which the qualities of aeroplanes could be judged, a standard which has hitherto been lacking. He asked Professor Jones if he could give, there and then, an actual numerical example of what an aeroplane of reasonable weight, horse-power, and so forth, must do to achieve the ideal. With regard to the curious fact mentioned by one of the speakers, that in the course of experiments he had found that the lift coefficient began to drop at much lower values at low values of Reynolds' number than at high values, Mr. Bramson asked if it were not possible that that was due to the departure from the boundary layer occurring sooner at low speeds, due to the relatively greater effect of the "adverse pressure gradient" near the trailing edge, than at high speeds. With regard to circulation, he said he had heard it stated that the "equivalent circulation" round an aerofoil would never start in the ideal fluid. He asked if that were so, and whether the idea of a circulation around an aerofoil was at all a sensible one. It was difficult to see how, if there were anything which could properly be called circulation, it could go round a sharp trailing edge, for instance, in any intelligible manner.

George H. Dowty (*contributed*): Following Mr. North's recent lecture, it is possible to compare the ideal aeroplane with present day standards and appre-

ciate the vast improvements which have yet to be made. The lecturer stated that progress towards the ideal streamline aeroplane has been slow and points out that the tendency to belittle minor gains is a wrong policy. The design of racing machines has produced a very clean type of aircraft, and here the designer is prepared to sacrifice practically everything in producing an ideal aerodynamic form. When speed is not the ruling factor, the question of maximum aerodynamic efficiency often takes second place to other considerations and justification is claimed for this because the increase in performance is small. If the existing gap between present designs and the streamline aeroplane is to be bridged then improvements, however small, cannot be neglected. One of the chief items of parasitic drag, and one to which Professor Melvill Jones has drawn attention in R. & M. No. 1115, is the undercarriage. The writer has been engaged on work in this direction for some considerable time, and during the last three or four years has confined his attention to methods for reducing the drag of this unit. Investigations have led to the same conclusions as those given by Mr. North, that the retractable undercarriage is not desirable because of the complicated retracting mechanism and the extra weight involved. Mr. North suggested the use of mechanical launching gear and, from his remarks, it is understood, a form of ski landing gear. This form of undercarriage is open to strong objections because it limits the operation of aircraft to stations equipped with launching gear and such machines would be at a great disadvantage in the case of a forced landing. There is also the question of ground manœuvrability and it does not seem reasonable to expect any sacrifice in this direction.

There is fortunately some scope for cleaning up existing forms of undercarriages, and it is on this basis that the writer has been working. The shock-absorbing member is usually located in the slipstream and it will be found that this member gives a resistance at least five times as great as a steel streamline tube carrying the same load. Further, the existing types of connections are very poor, the strut sections being broken at their points of juncture with the body. Such a condition must cause mutilation of the air flow at these points and give rise to additional drag (now termed interference). In order that these disadvantages can be obviated, the writer has designed several types of landing gears where the structure is rigid and the shock-absorbing mechanism lies within or adjacent to the wheel. The wheel with internal springing consists of an oil dashpot shock absorber, compression rubber springing and wheel brakes. This design of wheel has been in existence for two years, but it has only recently been favourably reviewed by wheel manufacturers. The Curtiss Company, of America, have produced a similar type of wheel, and they consider this undercarriage an attractive proposition. The shock-absorbing capacity of these wheels is better than that usually obtained with an oleo leg, because there are no limitations due to angular movements of the structural members. Conservative detail estimates show that the undercarriage drag can be reduced by 25 per cent. and the weight by 15 per cent.

G. T. R. Hill (*contributed*): This paper sets up for the first time a standard by which the performance of any aeroplane may be judged. Considering how far all designers are below that ideal performance, or "Jones performance," as it should now be called, serious objection cannot yet be brought against the standard on account of its being built up with the aid of what Professor Jones calls "a leaky theoretical argument." When the present drag of our military and commercial aeroplanes has been halved, it will be time to set the standard more securely on its base with the aid of aerodynamic knowledge which will then be available.

There appears no doubt that the rate of progress towards the streamline aeroplane is greater than that achieved by Nature in bygone ages in the design of birds and fishes, yet any elation which may be felt over this will be consider-

ably moderated by turning back some eighteen years to 1911, to the remarkable designs of M. Nieuport. There is a good description of these little aeroplanes in the old *Aero* of June, 1911; one of them which flew in the Gordon-Bennett Race of that year was fitted with an engine of only 28 h.p. nominally and with a wing area of about 170 square feet, it is reported to have attained a speed of 75 m.p.h. Looking at the photograph of the aeroplane it might be asked how could its resistance be brought down to the modern standard? It could be said at once that streamline wires could be fitted instead of round cables, and having said this, nearly all has been said.

Turning to the future, the fact cannot be ignored that as the resistance due to some parts of the aeroplane is reduced, the resistance of those parts left unaltered calls more and more loudly for attention. For this reason, it is difficult to foresee clearly the future of the air-cooled engine, in which perhaps four-fifths of the resistance is due to the turbulent air flow around the cylinders and one-fifth to the skin friction which necessarily accompanies the cooling effect required. Already the air-cooled engine is seen to be at a disadvantage in high speed racing aircraft, and in the usual course of events, characteristics of the racing craft of to-day are exhibited by the general purposes craft of to-morrow. How long will that to-morrow be in coming?

The " Jones ideal performance " stands out as one of the beacons towards which the designer must steer his painful way through the fog of detail work which necessarily surrounds him.

S. J. Davies and C. M. Whyte (*contributed*): The writers would like to thank Professor Melvill Jones for the excellent way in which he made a highly specialised subject both intelligible and interesting to those who, like themselves, are not conversant with the subject. They were particularly struck by the num-

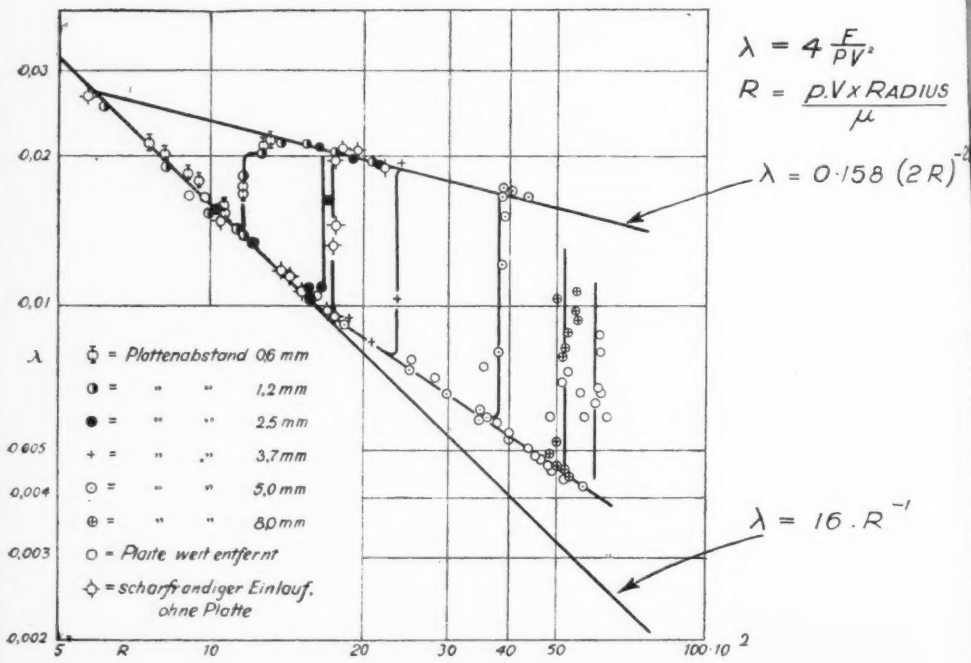


Fig. 5.

ber of points arising which threw light upon matters which had been obscure to them in an allied subject. It is possible that the following remarks, based on the study of flow through pipes, may in their turn be of interest to aeronautical engineers.

One of the most striking points brought out by the lecturer was that the majority of wind tunnel tests lie within the transfer region between laminar and turbulent flow. This fact must add greatly to the difficulty of interpreting the results, and it is remarkable that reliable figures have been obtained at all. For many years the similar transfer region in connection with pipe flow also gave rise to difficulty and led to much confusion of thought. And in fact it is only within the last few years that any certainty has been reached. The curves from flow in pipes are similar to those given in the lecture and the results of the former may help in interpreting the latter. The work of L. Schiller[11] has played a large part in demonstrating that the amount of turbulence in the fluid as it enters the pipe determines the value of the Reynolds' number at which the transfer takes place. Some of his results, which do not appear to have been published in this country, are shown in Fig. 5. The wide difference between the values of the Reynolds' numbers of the figure and those mentioned in the lecture are due to the fact that in the figure the Reynolds' number is that formed by the linear dimension of the cross section of the stream, while in the lecture the overall length of the body is used. The value of $F/\rho v^2$ in the figure is arrived at by the method used in the lecture, but it should be noted that it does not include the resistance at or near the entrance to the pipe. The various curves were obtained with increasing entrant disturbances, and show that the greater the turbulence at the inlet the smaller is the Reynolds' number at which the transfer takes place. There is, however, a limiting value, and no amount of disturbance will cause the transfer to take place below $R = 1160$. The relevant conclusion to be drawn from this is that, unless a high degree of turbulence is provided by artificial means, the results will neither be free from the influence of the unknown state of the fluid at entry, nor be consistent among themselves. With artificial turbulence it is possible to obtain points on the turbulent line for flows only slightly in excess of $R = 1160$. Further tests of Schiller show also that a moderate degree of roughness of the pipe does not influence the transfer.

The vertical transfer curves shown in the figure were obtained only when a length of pipe at least equal to 130 diameters is interposed between the entrance and the testing length. Under other conditions the transfer curve is less definite and becomes rounded in form and very similar to the dotted curve shown by the lecturer. The value of the Reynolds' number at transfer, however, remains approximately unaltered. The lecturer's method of calculating the form and position of the transfer curve is interesting, but he does not indicate the grounds for his implied assumption that the Reynolds' number, formed by the distance from the leading edge to the breakdown point, is constant during the transfer period. At first sight it might be expected to decrease progressively as the transfer curve is traced out in an upward direction.

The lecturer gave the impression that curvature tends to cause the boundary layer to become turbulent sooner. It would be interesting to know if there is any experimental evidence supporting this view. Curvature in pipe flow, provided that it is slight, appears to be without influence, but, as is shown by some tests about to be published, flow through a curved pipe, of which the radius of curvature is 15 times the pipe radius, is considerably more stable than in a straight pipe, and the transfer does not take place until about four times the Reynolds' number at which it would occur in a straight pipe.

The necessity for carefully regulated conditions when determining the resistance of complicated bodies is well illustrated by tests of a series of very

[11] Zeit. f. ang. Math. u. Mech. Vol. 1, p. 436, 1921.

rough pipes[12] carried out under somewhat indefinite entry conditions. These tests show certain striking anomalies. Two pipes, one definitely rougher than the other, were reversed in their resistances when tested over a certain range of Reynolds' number near the transfer region. This behaviour was characteristic, in this region, of the majority of the pipes tested, and persisted long after the flow had ceased to be streamline. The conclusion may be drawn that no reliance can be placed on the relative magnitudes of the observed resistances of two models of a complicated form unless the comparison is made at Reynolds' numbers many times as great as those of the transfer region, or, alternatively, unless the state of the fluid is completely known.

In connection with pipe flow the writers have found logarithmic plotting along both axes to be more convenient than the usual method of plotting $F/\rho v^2$ on log. R. Equations (1) and (2) of the lecture would then become straight lines sloping downwards. Plotted as they are, there is a false impression given, owing to the upward concave form of the curves, that they are tending to some constant value of $F/\rho v^2$ at high values of R. There is a further advantage of logarithmic plotting in that inaccuracies of test data of equal percentage are consistently shown at all parts of the diagram by equal displacements, so that undue importance is not attached to isolated tests which happen to fall on parts of the diagram where the scale may be relatively close.

Mr. SIMMONS (*communicated*): Professor Jones has to-night expounded a theory based on the assumption that the skin friction resistance of bodies of fine form follows one law at low values of Reynolds' number and another law at

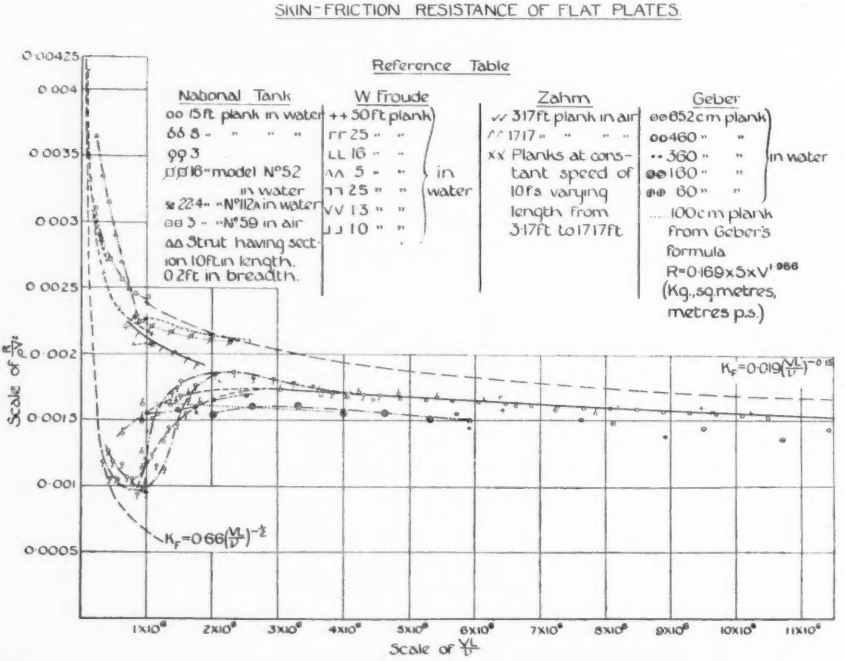


FIG. 6.

[12] Fromm: Zeit. f. ang. Math. u. Mech. Col. 3, p. 329, 1923.

high values. The underlying basis of the theory recalls the methods employed by naval architects for predicting the resistance of ships, founded on the skin friction data of flat plates. He would like to call attention to some results published some years ago by Mr. Baker[13] which he thought pertinent to the present discussion, and lent support to the new theory. These were shown graphically in Fig. 6. It would be observed that below a Reynolds' number of about 4×10^6 large discrepancies existed between individual observations made at the same value of VL/ν; although at higher numbers the results were more generally consistent. Commenting on the measurements made in air and water on similar models, he thought it was significant that the drag coefficient measured in the turbulent flow of the wind tunnel was higher than the figure obtained in the water experiments. If the initial turbulence in the free stream was a factor which influenced the flow in the boundary and thereby the drag of the model, then its effect appeared to be most marked below $VL/\nu = 4 \times 10^6$. He was, however, a little doubtful whether a stable régime of flow independent of initial disturbances was completely established at this value of Reynolds' number, since the Göttingen results exhibited by the lecturer, and shown by the dotted curve were in excess of the figures derived from tests on the largest planks. Finally, he referred to the similarity existing between the results for bodies of fine form in the transitional region and those obtained with pipes; and he thought it must be more than coincidence that the turbulence curve for flat plates could also be taken to represent the resistance of pipes at high velocities.

Reply to Discussion

The discussion has been long and interesting, and I wish first to thank all those who have taken part in it.

In reply to *Major F. M. Green*, the turbulence in the wind tunnel which causes early break up of the boundary layer is of a peculiar kind with a very small scale. I imagine that the scale of the turbulence in the open air is too large to have any influence on the boundary layer and I should not therefore expect differences on different days due to this cause. This opinion is not based on definite evidence. The figure given for skin friction drag coefficient is half the minimum profile drag coefficient, because the area involved in the former is that of both upper and lower surfaces of the planes, whereas the area involved in the latter is one surface only.

With reference to *Mr. R. McKinnon Wood's* remarks, I agree that it is probable that wind tunnels which have been deliberately given a small scale turbulence will be used in the future, but this matter has not yet been clearly thought out so far as I am aware.

Mr. Fage's statement that the Reynolds' number at which the boundary layer breaks up on a circular cylinder is of the same order as that for a flat plate, is most interesting. The effect of curvature on the point of break up of the layer is a matter which requires much further research. I shall be interested to see his results for thin Joukowski sections when they become available.

Mr. Walker's estimate that the speed of his aeroplanes lies between 60 and 67 per cent. of the streamline speed agrees fairly well with my rougher estimates. Since power varies approximately as V^3, the realised speed of 70 per cent. of the streamline speed corresponds to an expenditure of about three times the streamline power for a given speed. I am obliged to Mr. Walker for emphasising the fact that the ratio of actual to streamline power must not be regarded as an overall figure of excellence for the aeroplane. I had a warning to this effect in an early draft of the paper, but inadvertently omitted it in the final text. What the factor does show is how much power is being expended through defective

[13] Notes on Model Experiments, Collected Researches, N.P.L., 1916.

streamlining. I agree with his remarks about Mr. Lanchester and the induced drag theory; all the same, it was Prandtl who threw it into its present practical form.

Mr. W. L. Cowley finds it hard to reconcile my lecture with the available results which have been given by wind tunnels in the past. I do not understand his difficulty. The majority of results which have been used extensively have not been for perfect streamline bodies such as I am discussing. Also it must be remembered that the majority of full-scale results refer only to total brake horse-power, and that scale effects on the various parts may be confused with variations in airscrew efficiency or interference between airscrew and body. Only in a few instances, where research methods have been applied on the full-scale, do we know the drag independent of the airscrew, and even then the whole drag and not that of the parts separately.

There is one way in which apparent agreement can be obtained in spite of the existence of large scale effects; thus, the airship R.101 when tested on the model at a Reynolds' number of 2×10^6 gave $k_p = 0.0010$. This figure was almost exactly that which we should now predict for the full-scale with a wholly turbulent boundary layer at a Reynolds' number of 3×10^8. The apparent agreement here is entirely fortuitous, for at intermediate Reynolds' numbers the coefficient is much higher, as would be expected from the theory which I have advanced.

In Mr. Cowley's remarks upon pipes he appears to have forgotten that in a long pipe the boundary layer extends to the middle of the pipe. There is a relationship between the flow in pipes and the flow for a flat plate, but it is not so simple as Mr. Cowley appears to assume; it has been worked out by Professors Prandtl and Von Kármán (see my reply to Messrs. S. J. Davis and C. M. White for references).

I cannot understand Mr. Cowley's statement that the pressure distributions when resolved are equal to the whole drag; so far as I am aware this is incorrect for streamline shapes.

So far from the conclusions of my paper leading to a loss of faith in wind tunnel work, they will, I hope, increase its usefulness through removing some of the anomalies which have been well known for some years.

In answer to *Mr. Pye*, I have not included the Schneider Cup racers because, being seaplanes with floats, they are not comparable with the other aeroplanes in Fig. 3. It would be interesting to see them worked out accurately by someone in the Air Ministry who has the facts at his fingers' ends.

I agree with *Mr. Douglas* as to the importance of a clean external surface, but it is just possible that small projections may have less influence on the full-scale, where the boundary layer is probably turbulent in any case, than on the wind tunnel model, where the projections may convert a laminar into a turbulent layer.

Mr. Irving's suggestion that the boundary layer may continue laminar to higher Reynolds' numbers in respect of some shapes is very significant. We had already given some thought to this matter at Cambridge, but I left it out of this paper as being too speculative. This, I hope, will be the subject of future research.

His remarks about his experiments on skin friction in 1919, in which the skin friction coefficient fell on the curve for laminar boundary layer, are most interesting and I should like the reference. Dr. Stanton has also done experiments on the skin friction on thin rings similar to napkin rings, and has found the same agreement. There is no doubt that when the layer is laminar the agreement of the skin friction with Blasius' solution is thoroughly established.

Mr. Capon has clearly emphasised the main reason for the lecture, which was to show that an estimated sum of all conceivable drag reductions is so large

as to be worth going for, even if the separate contributions may in themselves appear small.

Mr. Scott Hall has doubts about my screw efficiency theory. I am not surprised at this, as it is at present the weakest part of the paper and is as yet unsupported by crucial experiment. The main conclusions of the paper, however, stand without it. The assumption certainly is, as he says, that the interference of screw on body balances the effect of body on screw, except for the increased power loss due to skin friction in the slipstream. Stated in this way, the assumption is certainly startling, but if my argument is carefully followed I do not see how the conclusion can be avoided. It is to be emphasised, however, that the argument applies only to ideally streamlined machines as defined in the paper.

In reply to *Mr. Bramson*, the suggestions in this paper cannot be considered as on the same plane as the Carnot theory of heat engines, but the practical outcome is similar—the provision of an ideal towards which to work. Whereas, however, the Carnot cycle is a precise theorem, my paper is more in the nature of an exercise in approximations.

I agree with *Mr. Dowty* that the landing gear is probably the greatest difficulty in the way of perfect streamlining, and I am interested to hear of his efforts to tackle the problem.

In answer to *Captain Geoffrey Hill*, the air-cooled engine is at present a distinct stumbling block to progress in streamlining, but I am not sure that it will always be so. Recent experiments in cylinder cowling are most encouraging and it should be remembered that, since the temperature of the cylinder is high,

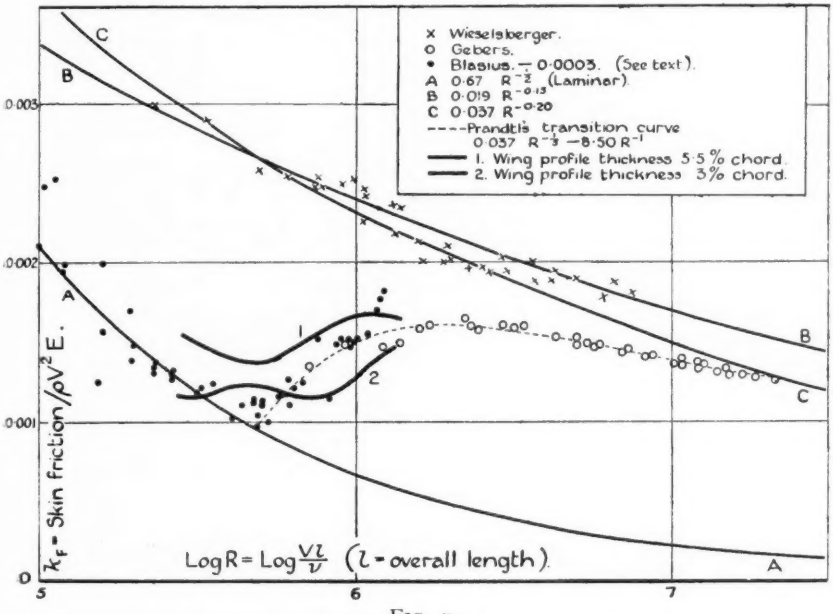


FIG. 7.

PROF. PRANDTL'S TRANSITION CURVE FOR SKIN FRICTION ON A FLAT PLATE.

Re-plotted from Reports 1 *and* 7, *Ergebnisse der Aerodynamischen Versuchsanstalt Zu Göttingen III. Lieferung.*

scientifically devised air-cooling systems, such as we have not yet got, may give rise to a relatively low resistance.

In reply to *Messrs. S. J. Davies* and *C. M. Whyte*, the relation between resistances to flow in pipes and the Reynolds' number formed from the pipe diameter was thoroughly worked out by Stanton and Pannel for both water and air, and a curve showing the relation of skin friction per unit area/ρV^2 to the Reynolds' number, for a very wide range including the critical region, was published in 1914.[11]

The comparison between flow in pipes and the boundary layer on a flat plate has been thoroughly examined by Von Kármán, of Aachen (Abhandlunger aus dem Aerodynamischen Institut an der Technischen Hochschule, Aachen, 1921) and by Prandtl (Ergebnisse der Aerodynamischen Versuchsanstalt zu Göttingen, 1927, Vol. III). Both these papers have been translated into English, the former by the A.R.C. as Report T. 2219 and the latter by the Air Ministry. In these papers a law, k_F varies as $(lv/\nu)^{-0.20}$ for flat plates with turbulent boundary layer is, with certain plausible assumptions, deduced from the law k_F varies as $(lv/\nu)^{-0.25}$ which is found to hold experimentally for pipes above the critical Reynolds' number. Fig. 7 is a composite diagram taken from two reports in the Göttingen publication mentioned above and roughly re-plotted to conform with Figs. 1 and 2. The curve $k_F=0.037\,(lv/\nu)^{-0.2}$ adopted by Prandtl, though it fits Gebers' points at the higher Reynolds' numbers, falls lower than the results from Froude and the N.P.L. given in Fig. 7. Hence for practical purposes I prefer his original formula $k_F=0.019\,(lv/\nu)^{-0.15}$, which gives a conservative estimate of skin friction drag for flat plates over the whole known range.

I do not understand the writer's difficulties with the transition curves. The only assumption regarding the point of breakdown from laminar to turbulent flow is that the point is uninfluenced by the portions of the plate behind it. This assumption must surely be correct for the flat plate; whether or not it is correct for the curved surfaces is shown by comparing the curves for k_F against R for the various streamline bodies with the theoretical transition curves for the flat plate.

To avoid unduly lengthening the paper, I did not give my methods of calculating the transition curves. The difficulty lies in deciding to what extent the laminar layer on the front part of the plate will influence the turbulent layer behind it. If it is assumed not to influence the turbulent layer at all, an upper limit is given for the coefficient of the average drag of the whole plate. If, on the other hand, it is assumed that the turbulent part in the rear behaves as though the whole layer were turbulent, a lower limit is given to the average coefficient. In forming the transition curves in Figs. 1 and 2 I worked out curves on both the above assumptions and took the mean between them. Prandtl, on the other hand, worked on the second assumption only in producing his transition reproduced curve in Fig. 7. The difference between my method and Prandtl's is not great, and I do not know which is nearer the truth.

The question of the effect of curvature on the stability of the boundary layer is still obscure, and will, I hope, shortly be the subject of experimental investigation.

I am aware of the advantage of complete logarithmic plotting, but I thought that on the balance the system which I adopted would be more easily explained to members of the audience who might not be very familiar with logarithmic plotting. The use of logarithms in connection with Reynold's numbers is easily explained as a convenient device for compressing a large range of numbers into a small space.

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[11] *Phil. Trans. Roy. Soc.*, 29/1/14, Vol. 214 A.

I am much obliged to *Mr. Simmons* for drawing attention to the available data contained in the figure accompanying his remarks. My attention had been drawn to the existence of this data by Sir Richard Glazebrook a day or two before the lecture, but I had not had an opportunity to consider it. Since the lecture I have secured Mr. Baker's original paper and have re-plotted the information on the same plan as Figs. 1 and 2 though on a larger scale, and the resulting

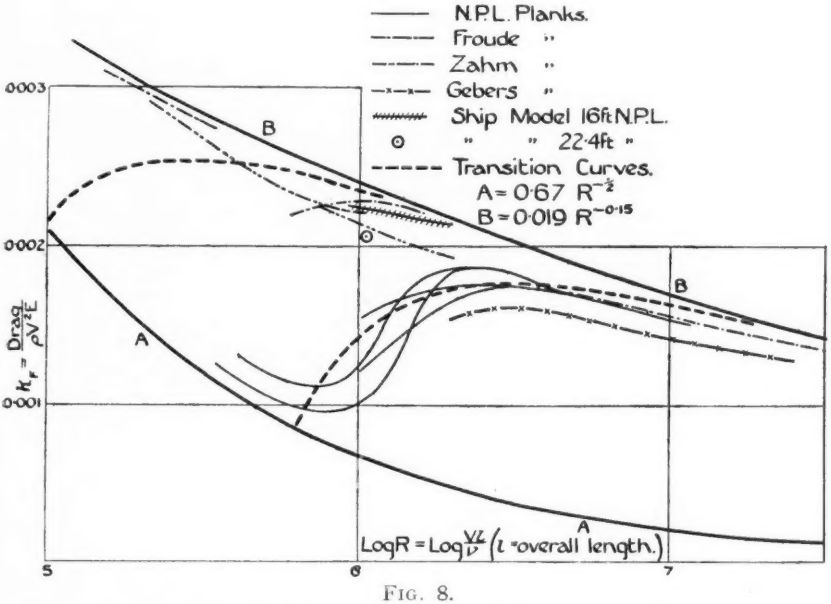


FIG. 8.

SKIN FRICTION EXPERIMENTS IN N.P.L. NAVAL TANK.

N.P.L. Collected Researches, Vol. XIII., 1916, *pp.* 103-129, *from Plate I. Re-plotted.*

diagram is reproduced herewith (Fig. 8).[15] It is evident, both from Mr. Simmons' figure and from mine, that the boundary layer in the N.P.L. tank experiments was breaking up at about $R = 6 \times 10^5$, and it is clear from my figure that at high Reynolds' numbers the curves of Froude and the N.P.L., which continue up to 4×10^7, lie within some 5 per cent. of the equation $k_F = 0.019\,(lv/\nu)^{-0.15}$. These latter data bring our knowledge of the skin friction coefficient of a flat plate up to a Reynolds' number of 4×10^7, that is to say, well into the region of Reynolds' numbers represented by full-scale aeroplane wings and bodies. They show that the formula which I used in my lecture for the skin friction is on the safe side in this region.

[15] For permission to publish Figs. 7 and 8 I have to thank the Aeronautical Research Committee, to whom I have submitted the diagrams as part of a report on this matter.

CIRRUS MARK III.

BY A. N. CAPLE.

Lecture before the Yeovil Branch, February 5th, 1929. (Abridged.)

It may be of interest to give a resumé of the Cirrus engine from the time of its conception up to the present period. The main idea actuating the designer was to produce an engine whose features would be reliability, low maintenance costs, and capacity to run for long periods with little more attention than a motorist gives to his car engine, and also so constructed that the average owner driver or motor mechanic could readily understand it and maintain it in perfect running order. The first Cirrus type engine was completed in May, 1925, and was submitted to the 100-hour Air Ministry type tests. These tests were successfully passed. The series of tests laid down by the Air Ministry Form A.P. 840 are of a most exacting nature.

A batch was immediately put into production. One of the first production engines was fitted to a light plane, the D.H. "Moth," and in June Sir Alan Cobham accomplished a flight from London to Zurich and back in one day, the distance flown being approximately 1,000 miles. In the same month Colonel the Master of Sempill flew from London to Dublin in a similar machine, the journey necessitating a 70 mile sea crossing. About this time the Light Aeroplane Club movement was inaugurated by the British Air Ministry, and all machines supplied were fitted with Cirrus engines.

The year 1926 saw further Cirrus achievements. The King's Cup race was won by Captain Broad and the Australian Air Derby by Sir Alan Cobham, both in Cirrus/Moths. In September of this year it was computed that Cirrus engines had flown over 1,000,000 miles.

The opinion of the Light Plane Clubs and private owners after approximately two years' experience under varying conditions was that still more power was necessary.

The Cirrus engine was re-designed in January, 1927. The Cirrus M. II. made its appearance, having just previously again been submitted to the Air Ministry type tests, and passing them successfully. The power of this engine was 80 h.p., compared to 60 h.p. of the Mark I. As a result of the experience gained with the Mark I. new features were incorporated in the design of the Mark II., to still further increase the reliability and lessen the cost of maintenance.

Before placing any number of these engines on the market another strenuous test was accomplished. Captain Stack and Mr. Leete, on two Cirrus Mk. II. Moth machines, flew from London to India (Karachi), a distance of 5,500 miles. This set us a record for light aeroplanes, the journey including a 250 miles sea crossing.

Throughout the year the list of successes was added to, to mention the most important:—

In April Major de Havilland, in a Cirrus/Moth, flew from Perth to Melbourne, a distance of 2,000 miles.

In May, Her Grace the Duchess of Bedford completed a tour of 4,500 miles through France, Spain and Northern Africa in a Cirrus/Moth piloted by Captain C. D. Barnard.

In August the King's Cup race was won for the second year in succession by a Cirrus/Moth, and the second machine was a Westland Widgeon, also fitted with a Cirrus engine.

The same month the longest light aeroplane non-stop flight was accomplished by Mr. Bert Hinkler in a Cirrus/Avian machine, when he flew from London to Riga (Latvia) in 10¾ hours—a distance of approximately 1,200 miles.

On the 1st September, Lieut. R. R. Bentley, of the South African Air Force, commenced a flight from London to South Africa (Cape Town), a distance of 8,000 miles in a Cirrus/Moth. This was completed by the 28th of the month. Lieut. Bentley was awarded the Brittania Trophy for 1927 in recognition of this remarkable flight.

There is one performance which cannot be passed without special comment, and that is the flight from London to Australia by Mr. Bert Hinkler in his Cirrus/Avian. It is of particular interest to record that the Cirrus engine used on this flight had been regularly used by Mr. Hinkler for a period of 15 months previous, and was that used on the London-Riga flight. Australia was reached in 15½ days, the previous time record for any type of machine being 28 days. The distance covered was 12,000 miles, or practically 800 miles per day.

In the early months of 1928 an opinion which seemed to be rather general was voiced, asking for still more power for the light planes. The outcome of this was the introduction of the Cirrus Mk. III., which developed 95 to 100 h.p. This increase has been obtained after numerous experiments, whereby a more efficient cylinder head and valve gear has developed.

General Description

The engine is of the vertical 4-cylinder-in-line air-cooled stationary type. This type of engine is usually much easier to instal, and lends itself more readily for the production of good cowling lines than the radial type of engine. The following general description is applicable to the Mark II. and Mark III. types.

Cylinders

Cylinders and cylinder heads are separate, the cylinders being of cast iron and the cylinder heads of an aluminium alloy with air-cooled fins cast on each.

The complete cylinder with head is secured to the crankcase by means of four studs projecting from the crankcase and passing through holes in the cylinder head.

In the cylinder heads special bronze seatings for the exhaust and inlet valves are screwed and expanded into position. The valve guides, which are a force fit in the heads, are of phosphor bronze.

Valves and Valve Gear

In the crown of each cylinder head one inlet and one exhaust valve are fitted. Both valves are operated by rocking levers, which are in turn operated by means of push rods in conjunction with the tappets located in the upper part of crankcase. The tappets are actuated directly by the camshaft.

The rocking levers are carried on a separate steel bracket, which is bolted to the platform provided on top of the cylinder heads.

The valves are of a special heat resisting steel, known as K.E. 965, and the valve springs are of the spiral coil type, secured by means of split collets and collars.

The timing of the valves when cold is as follows :—

Inlet opens 12° before top dead centre.
Inlet closes 70° after bottom dead centre.
Exhaust opens 70° before bottom dead centre.
Exhaust closes 28° after top dead centre.

The valve tappet clearances (cold) are as under :—

Inlet	.005 in.
Exhaust	.020 in.

Pistons

The pistons are aluminium alloy castings, the crowns of which are adequately reinforced on the underside by cross webs. They are fitted with three piston rings of cast iron. The lowest of these rings also acts as a scraper ring, insomuch as a groove is turned in the piston immediately below it, and a number of small holes are drilled around this groove through which oil can escape into the inside of the piston. This arrangement prevents any excess of oil on the cylinder walls from finding its way into the combustion chamber.

The hollow gudgeon pin is of the full floating type, that is to say, it is free to turn both in the housings in the piston and also the small end of the connecting rod.

End play beyond the maximum tolerance is prevented by a spring circlip at each end, which is inserted into the housing and allowed to expand into an annular groove, into which it is only forced more tightly when pressed by the end of the gudgeon pin.

Connecting Rods

The connecting rods on the Mark II. are made from duralumin forgings, but the Mark III. are forgings of special "Y" alloy. This change of material follows the general trend of engine design, and we are of the opinion that, in view of the higher stresses carried by the Mark III. connecting rod, it is more suitable material.

The cap of the big end is registered to the rod by two bolts, and the big ends are fitted with phosphor bronze white metal lined bearings. A dowel is fitted to the cap and cap half of bearing to prevent rotation.

Crankshaft

The four-throw crankshaft is of solid construction, and is integral with the propeller shaft.

The complete shaft rotates in five bearings, *viz.*, three die-cast white metal centre bearings, and a roller bearing at each end. The caps for the roller bearings and centre bearings are carried by the upper half crankcase, and are secured by studs and nuts. The caps are steel, and of robust design.

In addition to the bearings mentioned above, a radial thrust bearing is fitted on the extension of propeller shaft.

Crankcase

The crankcase is an aluminium alloy casting, divided along the centre line of the crankshaft. The upper portion is stiffened by three transverse webs, which also form the housings for the centre bearings. Housings are provided in the upper portion for carrying the camshaft bearings and also the upper oil pump spindle bearings. Provision is also made for securing four bearer or supporting feet. Housings for the ball bearings carrying the timing gears are provided in the upper half and timing gear cover. On the front wall above the nose a small casting is secured, which acts as an oil filler and to which breather pipes are attached.

The lower half, beyond forming in itself a cap for the radial thrust bearing, merely acts as an oil sump and cover for the moving parts.

Camshaft

All the valves are operated by a single camshaft. It is supported in four bearings, the rear being a large ball and the remainder phosphor bronze. The shaft is driven off the end of the crankshaft through the medium of steel gears, supported, as already mentioned, between ball bearings housed in the top portion of crankcase and timing gear cover.

A spiral gear integral with the camshaft and situated centrally drives the oil pump spindle.

Provision is made for coupling the tachometer drive to the rear end of the camshaft.

Induction Manifold

The induction manifold is of steel, with branches to each inlet valve port. A heated muff is provided in the central section, a pipe being taken from the exhaust manifold so that the exhaust gases can circulate therein. The joints between the induction manifold to cylinder heads and carburettor are made with Hallite washers.

Lubrication

The lower half of the crankcase (usually termed the oil base) holds 18 pints of oil, which is sufficient for about 7-8 hours flying at maximum consumption. It is usual, however, to put in about 12 pints, which is sufficient for about 4-5 hours.

The oil pump is arranged at the lowest part of the base, so that it is always flooded or self-primed with oil. The oil pump, which is gear type, forces the oil through a gauze filter, which is arranged horizontally just above the pump, thence through the main delivery pipe to the oil gallery arranged on the left-hand side of the engine. The oil gallery is connected to passages drilled in the top of the crankcase, which runs to the centre and intermediate bearings, so that the oil is forced under pressure direct to each bearing.

The crankpins are lubricated as follows. An oil thrower or banjo fitting is fastened to the crankwebs, and the oil overflowing from the adjacent main bearing is caught into the interior of the thrower and forced out by centrifugal action to the crankpin, the excess of oil thrown off the crankpins lubricates the other parts of the engine, some portion of it going into the cylinders to lubricate the pistons and gudgeon pins. The camshaft, being open to the interior of the crankcase, gets sufficient oil thrown on it to lubricate its bearings and also the valve tappets and guides. Quarter-inch pipes are connected to the ends of the oil gallery, the forward pipe leading to the propeller shaft radial thrust race, and the rear pipe to the timing gears, so that the oil from it lubricates the teeth of the gears.

A gauze strainer is fitted in the upper part of the base, through which all the oil passes after doing its work in the engine, and then drops to the bottom of the base, where it is used again.

A level indicator, which is graduated in quarts, is placed down the side of the base, so that it can be easily withdrawn and the level of oil ascertained.

Oil Pressure

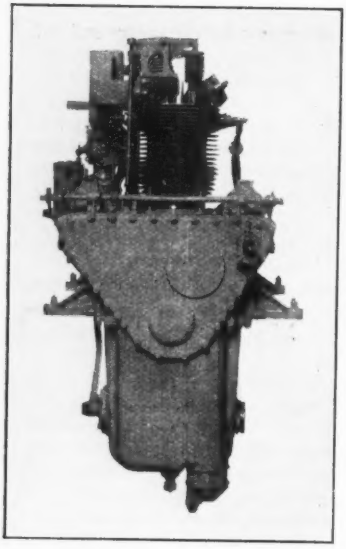
Provision is made for connecting up to the oil pressure gauge through a three-way piece fitted in the timing gear cover.

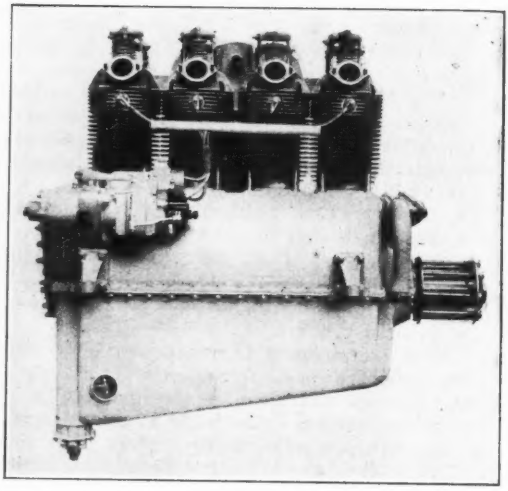
Ignition

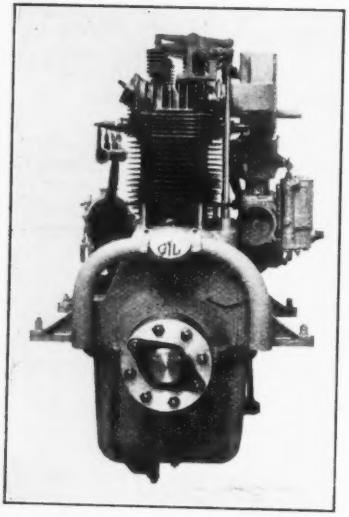
Two sparking plugs are fitted in each cylinder, and they are placed at opposite sides of the combustion head in order to promote the maximum rapidity of inflammation of the mixture. They are fired by two four-cylinder high tension magnetos, which are mounted in tandem.

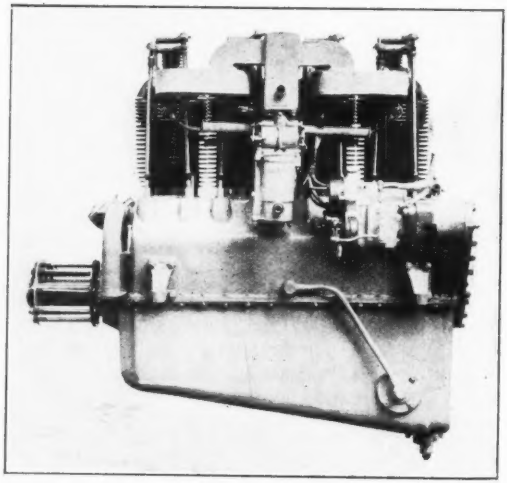
The forward magneto is of the platform type, and is driven through the medium of a Simms Vernier Coupling and Impulse Starter, while the rear magneto is driven through a similar Vernier Coupling, but no Starter.

The virtue of an Impulse Starter is to obtain an intense spark even when the engine is turned over by hand, and it has been found that the type fitted is very successful in achieving this object, and for facilitating the starting up of the Cirrus engine by hand.









Carburettor

The type of carburettor fitted is the "Claudel Hobson" (R.R./C.H.).

Before closing this lecture, I should like to refer to a new engine that has been developed recently by us, and which has also unofficially passed its type test.

As the information is not public, it must be treated with a certain amount of reserve, but a few details regarding the design may be of interest to you.

It is a four-cylinder-in-line engine, but differs from its predecessors considerably in outline and arrangement.

To mention a few points, the magnetos are placed one on each side of the crankcase, with the contact breakers facing forward, to facilitate adjustment when necessary.

The oil sump is so shaped to more nearly conform to the accepted design of fuselage, and so enable a larger quantity of oil to be carried when necessary.

Force feed lubrication is adopted on all crankshaft bearings, including connecting rod big ends.

The over-all length of the engine is slightly shortened, and will facilitate its installation in most types of machines.

The cylinders are deeply spigoted in the crankcase and the cylinder head is similar in design to the Mark III. type previously described, and which to us has proved so satisfactory.

The bore and stroke are respectively 114 m.m. and 140 m.m., while the weight is little more than Mark III.

The oil pump is fitted at the back end of the sump in its lowest position, and there is only one external oil pipe, which passes the oil from the pressure filter up to the crankcase, and thence to the main gallery pipe inside.

An interesting feature of this oiling system is that the pressure release valve is situated in the oil pump itself, in such a manner that the oil pressure should remain absolutely constant under all conditions of speed, oil viscosity and temperatures, thus affording, by means of the pressure gauge, a definite indication that the oiling system is working satisfactorily or otherwise.

An experimental engine has been tested for a considerable period, and the results obtained so satisfactory that it was offered to the Air Ministry type test, and, as mentioned before, it has successfully passed its test, unofficially.

The power obtained at normal speed, namely, 1,900 r.p.m., is approximately 105 b.h.p., while the power at 2,100 r.p.m. was over 112.

At the normal speed, the mean effective pressure is in the neighbourhood of 125 lbs. per sq. in., and when it is considered that the compression ratio is only 5.1, the figure 125 is really excellent.

NOTES ON THE AERONAUTICAL SHOW HELD IN CHICAGO, U.S.A., DECEMBER 1ST TO 9TH, 1928.

BY F. S. BARNWELL, O.B.E., F.R.AE.S.

Paper read before the Bristol Branch, February 28th, 1929. (Abridged.)

The short paper which I am inflicting upon you to-night purposes to be on American aeroplanes. For brevity, however, I have found it necessary to confine myself to a somewhat cursory description of such American machines as I saw exhibited at the recent Aeronautical Show, held in Chicago from the 1st to the 9th of December, 1928.

The official designation of this Show was that of "International Aeronautical Exposition," but I noticed only two "foreign" exhibits, a D.H. Gypsy Moth, and an Avro Cirrus Avian.

From such records as I made at the time, I noted 62 aeroplanes by 44 different exhibitors.

Of these 62 machines, 31 were monoplanes and 31 biplanes (among these the Moth and the Avian).

Of the 31 monoplanes, 3 were "Parasols," 15 were of high-wing type with external bracing struts, 9 were of high-wing "Cantilever" type (*i.e.*, no external bracing), 1 was of low-wing type with external bracing struts, and 3 were of low-wing "Cantilever" type.

The biplanes were practically all of conventional present day practice, *i.e.*, braced by tween-wing struts and lenticular section h.t.s. wires. "Incidence" bracing, however, consisted in most cases of one strut instead of a pair of wires, and one or two biplanes used struts only for the external bracing.

Of all 62 machines only 4 were Service machines:—The Chance Vought "Corsair 2-seated Fighter," the Martin "Bomber," the Hall Aluminium Company's "Pursuit Ship," and the Keystone "Bomber"; all biplanes. The first three had each one radial air-cooled engine, and the last (the Keystone "Bomber") a pair of "Liberties."

Turning now to the commercial types:—Of the 58 machines I noted, 54 were single-engined and 4 multi-engined, the latter being 2 tri-motored Ford cabin monoplanes, 1 Fokker tri-motored cabin monoplane, and 1 Sikorsky twin-engined biplane cabin Amphibian. Of the 54 single-engined commercial machines, 1 (a Fokker) was a monoplane cabin Amphibian, 19 were cabin land monoplanes, 3 were cabin land biplanes, 1 was a Boeing biplane air yacht, 1 a Loening Amphibian biplane air yacht, 7 were open land monoplanes, and 22 open land biplanes. By "open" is meant a "normal" body with open cockpits.

Only two machines in the Show had "pusher" airscrews, these being the Fokker Amphibian and the Boeing air yacht,—all the rest had tractors.

As regards the type of engine installed, 47 machines had air-cooled radial engines, 11 had water-cooled Vee engines (Curtis Ox or Wright Hispanos), 3 had 4-cylinder vertical in line air-cooled engines, whilst 1 machine, the Driggs "Skylark," had a new 4-cylinder in line inverted air-cooled engine.

The foregoing tabulations are somewhat lengthy and dry, but seem advisable as a pointer to the present trend of over-all design in America.

One rather concludes from them that the Americans are out to produce sound machines quickly, and with minimum of aerodynamic experiment, as such a large percentage of the machines exhibited were "conventional" as regards the disposition of their elements—this seems good policy in engineering

—probably better results are achieved by development of well-established types than by playing about with radical alterations.

I should say that only 3 machines in the Show might be described as "abnormal" in their lay-out, these being the new Fokker Amphibian, the Loening air yacht, and the new Sikorsky Amphibian.

The Fokker Amphibian is a metal-hulled flying boat with the standard Fokker wooden monoplane wing on top of the boat, the single air-cooled radial engine (Pratt and Whitney "Hornet" or Wright "Cyclone") is mounted on struts on top of the wing and drives a pusher airscrew. Short "stub-wings" project from the lower "corners" of the boat body; these serve as wing floats and also as fairings for the landing wheels, which can be retracted into recesses in the tops of the "stub-wings." Frankly, I do not think the machine looked a good flying proposition. The line of airscrew thrust seems too high, the stability on the water seems doubtful, and one would expect a very high water drag from the "stub-wings." I do not know whether the machine has actually risen from water, but I doubt if it could do so at full load.

The Loening air yacht, though of unusual appearance, needs no description here, as it is already well known and has been in service for some years.

The new Sikorsky Amphibian is a rather curious machine, and can hardly be described as beautiful to look at, but I believe that its performance is quite good. It is a twin-engined biplane flying boat. The roots of the short span lower wings are anchored to the sides of the boat, and the long span upper wing is mounted on a series of struts. The tail unit is carried on a pair of tail booms, which project backwards from the top wing; a pair of struts is fitted between the tail end of the boat and the tail booms, thus supporting these booms near their after ends. The twin-engine nacelles are mounted on struts between the upper and lower wings and carry tractor airscrews. A pair of wing floats are mounted on struts below the bottom wings. The axles and radius rods of the land undercarriage are hinged to the sides of the boat (above the chine line) and the shock-absorber legs are attached, at their lower ends, to the ends of the axles (*outside* the wheels), at their upper ends to the under sides of the engine nacelles. Oleo springing is employed in these legs, which are, of course, telescopic, and the telescoping (for lowering the wheels for land work or raising them for water work) is also done by oil pressure, actuated from the pilot's cockpit.

Altogether the machine looks rather a mass of struts, and the short tailed boat and tail booms are not pretty, but the machine is undoubtedly a light, rigid and practical structure.

It being quite impracticable to describe all machines in the Show, I must confine myself to the few that particularly caught one's eye, either for attractiveness of over-all lay-out or for novelty of structure.

The machines that appeared to me the best designs for small commercial passenger carriers were the Buhl Air Sedan and the Stearman Four-plane Coach. Both are small neat cabin biplanes, and I preferred them to the monoplanes of the same class, either "Cantilever" or strut placed. My reasons for the preference are that these biplanes are rather lighter than the strut braced monoplanes, considerably lighter and more rigid than the "Cantilever" monoplanes, and, as far as one could judge, the parasitic drag of their external wing bracing should be less than that of the strut braced monoplanes.

I know I am talking against the prevailing fashion, but I think I am correct. Whether the amount of blocking out of the passenger's downward view by the lower wings of a biplane is of importance or no, is a matter hard to assess fairly, but the advantages of light weight and rigidity should not lightly be sacrificed on its account.

One point in which I preferred the Stearman to the Buhl was the undercarriage. The upper ends of the shock-absorber legs of the Stearman were

anchored to the upper corners of the fuselage, whilst in the Buhl they were anchored to the lower corners; hence I should judge the Stearman to be more stable on the ground.

Probably the finest piece of detail work in the whole Show was the Ford-Stout trimotor monoplane. The over-all lay-out of this machine is similar to that of the trimotor Fokker, so need not be further described. The structure of the machine is entirely of duralumin, the covering of wings, of body and of tail surfaces being of corrugated sheet (like the Junker machines). One imagines that an enormous number of jigs and tools must have been made to produce this machine, otherwise the amount of hand labour in its construction would be prohibitive; but certainly the detail design and construction is of a high order.

The Hamilton all-metal monoplane again is similar in over-all lay-out to the single-engine Fokker, but its construction is all of steel and duralumin. The covering of wings, body and tail surfaces is of corrugated sheet duralumin, whilst the internal framing structure is mainly of duralumin, but partly of steel in heavily stressed parts. This machine struck one as being a sound, well-made job, but on the heavy side.

The Hall Aluminium Aircraft Corporation's "Pursuit" biplane and the Martin "Bomber" were the only aircraft shown in skeleton.

The former is a single-seated tractor biplane, with strut-braced wings. The one actually shown had been in service for some time, and had been stripped of its fabric covering and put into the Show mainly to demonstrate the durability of the material. Its structure was built up of aluminium alloy, and presented no unusual features. The metal appeared to be coated with varnish of some sort, and there were no obvious signs of corrosion or of cracking.

The Martin "Bomber" is a large single-engined tractor biplane for carrying bombs or a torpedo. The structure of wings and fuselage is mainly built up of special sections of duralumin, designed by the constructors, and made, I believe, by the American Aluminium Co. Fabric covering is used. I was not greatly impressed by this machine; the structure seemed complicated, and yet the material did not appear to be utilised to the best advantage.

I mention another aeroplane, because it was rather amusing—the Heath "Super Parasol," America's most popular sport plane (I quote from the firm's leafllet). This is a tiny, single-seated, tractor-screw "Parasol" monoplane. It is fitted normally with a Henderson motor bicycle engine, is advertised to have a top speed of 70 m.p.h., a stalling speed of 28 m.p.h., and to sell complete at about £200. It is also advertised that "you can build this ship" for about £42, presumably without engine or airscrew. It was an interesting little machine, but one rather wonders what will happen if many people attempt the £42 building effort!

Finally, mention should be made of an aeroplane just produced by the Keystone Corporation—the Keystone "Patrician." This machine should have been on exhibition, but was unfortunately damaged by fire shortly before the Show opened, and could not be repaired in time. It is a large high-wing strut-braced monoplane, engined with three Wright "Cyclone" engines, each of 525 h.p. It has a cabin with seating for 20 passengers, and is advertised as having top speed of 155 m.p.h. and an initial climbing rate of 1,150 ft. per minute, with a pay load of 3,880 lbs. It has a wing span of 90 ft., and a total loaded weight of 15,000 lbs.

It is also advertised as the largest overland transport aeroplane in the world, but as the "Argosy" is 18,000 lbs. and is a 90 ft. span biplane this claim is not quite accurate.

Briefly now to consider separate components:—

Airscrews

The majority of airscrews exhibited were all-metal two-bladers, with solid forged light alloy separate blades held in a divided hub of forged steel. A few three and four-bladers of the same type also were shown.

These screws are made by the Steel Airscrew Co., and by the Hamilton Co. From what I could learn at the stands of the makers, the blades are largely shaped and finished by hand, which sounds rather surprising in a machine-tool country like America.

A two-bladed airscrew suitable for the Wright "Whirlwind" sells at about £90 in metal, a wooden airscrew at about £25. The metal airscrew is about twice the weight of the wooden one, and, given equally good design, one would expect the metal airscrew to be of slightly higher efficiency at top speed and of slightly lower efficiency for climbing; hence the metal airscrew has got to prove greatly superior durability to justify its 250 per cent. increase of price and 100 per cent. increase of weight. I think I am correct in stating that not a single machine in the Show was fitted with an engine with geared-down air-screw drive. This probably accounts largely for the popularity of thin solid metal bladed screws, the biggest advantage of this type being that the *loss* of efficiency at high speeds is as low as can be achieved.

What will happen when geared engines are installed? One rather wonders. America is already developing geared engines, and our experience is that air-screws, when comparatively large and slow-running, are prohibitively heavy with solid light alloy blades, and no more efficient than wooden screws.

One interesting airscrew was shown on the Hamilton Co.'s stand. This screw had a "one-piece" steel hub, and the cylindrical roots of the duralumin blades were bored and tapped out and screwed on to the spigot arms of the hub, in distinction to the normal method of clamping the blade roots between the two halves of a divided steel hub. The method is neat, simple to produce, and relatively light, but is open to grave suspicion as to strength; the representative told me that this type was only produced for low powered engines, such as the Curtis "Ox."

As regards airscrews other than metal. These were exhibited in the normal laminated type, the favourite timber in America being apparently "quartered" white oak; they were also exhibited built up in many layers of thin veneer and moulded up in "Micarta." One was not particularly interested, as none of the types seemed better than our own practice. It was puzzling to note, however, that all wooden airscrews in America appear to be of the "straight trailing edge" type, a blade shape discarded now for years in England as being bad for torsional stresses,—the type of stress which wood is worst qualified to withstand.

Wings

The majority of machines exhibited had normal wood-built fabric covered wings; two spruce spars and built-up spruce ribs. The use of thin ply-wood round the leading edge of the wing, to retain the correct nose-shape, was almost universal.

The Americans seem to favour sections of what might be called "semi-thick" type, practically all machines having wings of Clark Y or Gottingen No. 397, or some other form very similar. It would appear, therefore, that the stresses due to terminal nose-diving are largely ignored.

The most fashionable type of wing structure was the monoplane with external bracing struts. Personally I have never felt much love for the type, and none of the examples exhibited at Chicago appreciably altered my mental attitude. To me they appeared frankly as rather messy jobs, big struts with smaller struts to support them, incidence bracing wires, etc.

Control Surfaces

Control surfaces of all shapes and sizes were exhibited, and one was rather driven to the conclusion that questions of stability and control were treated largely by guess work or rough empiricism. I would venture the opinion that a fair proportion of the machines would prove unstable and of insufficient control power near stalling speed. A number of machines had " Bristol-Frise " type of balance on the ailerons, and I noticed that a special light form of ball bearing was incorporated in the aileron hinges of several.

Undercarriages

Possibly in the undercarriage the difference between average American practice and average British practice is most strongly marked. The majority of British machines still retain the straight axle, which gives, generally speaking, the lightest and lowest drag form of undercarriage; again, nearly all modern British machines have oil-damped shock-absorbers. Practically all American machines have the divided type of undercarriage, and only a small percentage employ oil-damping. Many American machines still fit tension springs of stranded rubber shock-absorber cord, which has been discarded in England as too indeterminate and too perishable.

Most American undercarriages were good in that the track was wide and the upper ends of the shock-absorber legs widely spaced, but to achieve the latter end many machines had pyramids of struts projecting from the body, to which to anchor the upper ends of the legs.

A few machines in the Show were fitted with a very neat form of oleo-pneumatic leg, but this was a proprietary article, and the makers were not showing it themselves, so I was unable to get any details of it.

Practically all American machines now have brakes fitted to the main wheels of the undercarriage. " Bendix " internal expanding articulated shoe brakes fitted into " Bendix " duralumin disc wheels were universally fitted, with the exception of the Ford machine, which was fitted with its own hydraulic brakes. The brakes may be actuated either simultaneously (for braking), or independently (for manœuvring on the ground). They were generally operated by secondary foot pedals mounted on the main pedals of the rudder control, but some were actuated by a pair of side-by-side hand levers. Certainly, from what I saw of them in operation at the Chicago Air Port, the brakes on American machines seem to function extremely well, and to improve enormously steering on the ground.

Tail wheels, instead of tail skids, were fitted on the majority of machines. In a certain number of cases these wheels were small things moulded up of " Micarta " and appeared to be fitted more because they could be called wheels than because they could serve efficiently as such; but most of them were reasonable pneumatic-tyred affairs. One is rather divided in opinion on the question. Certainly a tail wheel ensures much less effort in pushing a machine about a smooth surface, and presumably (if reasonably large) it does less damage to an aerodrome surface, but against that it is heavier, more expensive to make and to keep in repair, and of higher drag; moreover, it is not a particularly pleasing excrescence to look at. Given brakes on the main undercarriage wheels it is probably better to have a tail wheel on large machines, but for smallish machines that habitually land on grass surfaced aerodromes, one feels that a good tail skid is, on the whole, preferable.

One should note in this connection that many aerodromes in the States are provided with run-ways, radiating from the centre of the aerodrome. From these run-ways machines have to get off, and on to them they must land, as the rest of the surface is too rough. As these run-ways are comparatively smooth and hard, one can understand that a tail wheel is preferable to a skid for work on them.

Fuselages

Fuselages are almost universally built up of steel tubes welded together. There were a few examples of other forms of metal construction, but apart from the Ford and the Hamilton, which both incorporated metal covering as well as framing, the built-up frames were not particularly interesting, and I do not think the design and construction were as good as that in modern British machines.

The welded-up jobs are, of course, of great interest and great controversy; rightly or wrongly, one cannot help being somewhat afraid of the practice, and, of course, the necessity to use a comparatively low grade steel entails a certain increase of weight.

The welded-up job is ideal from the drawing office point of view; I should say the drawings would only take about one-tenth of the time of those for a fuselage with articulated joints. The welded-up job, again, is a most simple method by which the shops can produce an experimental fuselage. Again, as the fuselage structure averages about 8 per cent. only of the total weight of an aeroplane, one should not, perhaps, make too much fuss about a 10 or 15 per cent. alteration of this fraction.

But one feels about welded-up construction that it is not a real production job. The same amount of skilled labour must be expended on each one of any number of fuselages—for welding on the structural members of an aeroplane must be retained at a very high level of excellence.

If one can entertain the fantastic idea of being required to produce (for sale) thousands of fuselages from the same design, then I believe that a good form of rivetted and bolted-up job might be cheaper than a welded-up job, as well as being lighter, more reliable and easier to repair.

The tube employed in America for most of the welded fuselages is solid, drawn from a chrome-molybdenum steel of about 42 tons per square inch ultimate tensile strength.

Controls

The flying controls fitted to many of the machines seemed of rather poor design and of rather rough workmanship—some did not look too strong; accessibility and convenience for inspection and lubrication seemed to have received little consideration. Perhaps this is a rather brusque and unfair generalisation, but it is the impression left upon me.

Many of the pulley wheels for the control cables were made of moulded "Micarta"; nearly all were of rather small diameter.

Most rudder controls were by foot pedals, and the majority of these were of stirrup form, *i.e.*, hinged at their upper ends; probably this form of rudder control is favoured as convenient for secondary brake pedal attachment and as occupying less space than the foot lever generally employed in British machines.

One or two machines had dual hand control, consisting of a central cranked "joystick." This stick could be locked into its bottom socket, with its cranked upper end either to starboard or to port, thus giving the control to starboard or port pilot respectively. This is certainly a simple and cheap way of achieving the object, but one wonders whether much thought had been given to the enormously increased stresses thereby induced in the lower end of the stick.

I did not notice a single adjustable seat or foot control, by "adjustable" being meant capacity for adjustment by the pilot in the air.

Instruments

The installation of instruments was, generally speaking, extremely neat. Most machines were fitted with instruments by the Pioneer Co., nicely grouped on a small panel prettily finished in "crystallising lacquer," the space occupied

by the instruments being less than half of that on a normal British machine. The instruments themselves were smaller than standard British ones, and more uniform in size. The fact of the small size and uniformity of size, though excellent for installation, probably make it considerably more difficult for a pilot to note quickly any particular reading he may require.

Some of the compass positions were bad, inaccessible and hard to see. One or two machines had the compass mounted behind the pilot's head, and he was expected to watch its reflection in a small mirror mounted above the dashboard.

I noticed one or two petrol gauges of the "glass-tube" type, but made in what I was told was transparent moulded "Bakelite"; there seem great advantages in such a material if it be petrol proof and practically unbreakable: one could see that it was quite reasonably transparent.

Nearly all American machines are fitted with engine starters. The favourite is the "Eclipse Inertia." This form of starter consists essentially of a small flywheel (about 6 in. diameter and weighing about 7 lbs.) which may be coupled, via a friction clutch and a train of reduction gears, to the rear end of the crankshaft of the engine. The clutch can be engaged and disengaged by a lever in the pilot's cockpit. The flywheel can be spun up to about 12,000 r.p.m, either by hand turning gear (in one form of the starter), or by a small electric motor (in another form).

The whole outfit weighs about 24 lbs. in the hand form, or about 30 lbs. in the electric form. It is an extraordinary neat and compact mechanism, and appeared to function very effectively.

Finish

As regards the general finish of American machines, this might be described as of a very high order, superficially; the finish on the fabric and metal work was beautiful, and the interior furnishings and fittings of cabins were generally excellent in appearance and well thought out as regards comfort and convenience. But as regards finish from an operational point of view, most of the machines were quite bad. All fabric appeared to be sewn on permanently, there appeared to be very few inspection panels, most aluminium fairing and engine cowling (though beautifully shaped and finished) was fastened on in most antiquated manner; woodscrews seemed largely to be employed, our old enemy the "skewer," or else sort of "hairpins" pushed through studs.

One or two pretty little cabin machines had celluloid windows and windscreens, all permanently fastened in, the pilot being practically enclosed in celluloid.

One rather gathered that pilots and maintenance engineers are easier to please in America than here, or else that there will be a lot of violent emotion expended if and when many of the machines get into service.

As regards prices of a few complete aeroplanes:—The "Waco 10," a 3-seater open cockpit biplane with 220 h.p. Wright "Whirlwind" engine costs about £1,550 with normal installations; £65 extra is charged for metal airscrew, £57 extra for an eclipse hand-operated engine starter, £54 extra for "Bendix" brakes.

The 2-seated "Air Trainer Parasol" monoplane, with 60 h.p. Le Blond engine, costs complete about £600.

The "Cessna Cantilever" wing monoplane with closed cabin for pilot and four passengers, with "Whirlwind" engine, costs about £1,850.

The "Consolidated Husky Junior" 2-seated open cockpit training biplane, with 110 h.p. Warner "Scarab" engine, costs about £1,350.

The "Monocoach" 4-seated cabin biplane, with 170 h.p. "Velie" engine, costs about £1,350.

The "Stinson Detroiter" 6-seater cabin monoplane, with "Whirlwind" engine and complete equipment, costs about £2,600.

As regards performance :—

The maker's figures tend to be optimistic; this fault is perhaps not confined to American constructors.

The "Ryan Brougham" cabin monoplane, with 225 h.p. "Whirlwind" engine, is advertised as having top speed of 125 m.p.h. and landing speed of 49 m.p.h., at a weight of 3,600 lbs. Official records give this machine a top speed of 116 m.p.h., and, as its wing loading is 12.8 lbs. per sq. ft., its landing speed cannot be appreciably less than 57 m.p.h.

The "Bellanca" cabin monoplane, with "Whirlwind" engine, is advertised as having top speed of 126 m.p.h., and landing speed of 46 m.p.h., at a weight of 4,050 lbs. Official records do give it a top speed of 126 m.p.h., but as its wing loading is 14.8 lbs. per sq. ft., its landing speed cannot well be less than 60 m.p.h.

The "Fairchild 41" cabin monoplane, with "Whirlwind" engine, claims 130 m.p.h. high and 49 m.p.h. landing speed, at a weight of 3,000 lbs. Official records give a top speed of 113 m.p.h., and, as the wing loading is 15 lbs. per sq. ft., its landing speed cannot well be less than 62 m.p.h.

The "Stinson Detroiter" cabin monoplane, with "Whirlwind" engine, advertises 125 m.p.h. top and 56 m.p.h. landing speed. Official records give a top speed of 117 m.p.h., and, the wing loading being 15.4 lbs. per sq. ft., the landing speed cannot be appreciably less than 63 m.p.h.

The Ford Co. does seem to give reasonably accurate performance figures; in fact, the catalogue of this firm's aircraft is one of the few good ones I obtained at the Show. They advertise their tri-motor monoplane, with three "Whirlwinds," a total weight of 10,000 lbs. and a wing loading of 123/4 lbs. per sq. ft., to have a top speed of 114 m.p.h. and a landing speed of 55 m.p.h. Official records give this machine a top speed of 118 m.p.h., and at the wing loading the landing speed should be about 58 m.p.h.

It is interesting to compare the officially recorded top speeds of these few examples with top speeds calculated by using the standard British airworthiness equations for normal aeroplanes :—

$(V/\sqrt{\omega}-25)^2=16.6\ (N/\sqrt{\omega}-11.8)$ for single engined.
$(V/\sqrt{\omega}-23)^2=15.7\ (N/\sqrt{\omega}-11.6)$ for multi-engined.

Machine.	*Recorded Speed.*	*Calculated Speed.*
Ryan	116	132
Bellanca	126	121
Fairchild	113	136
Stinson	117	111
Ford	118	119

From these figures it would appear that the top speeds of American machines do not show them possessed of unusually high efficiency.

Finally a note as to the weight of one American machine, the Ford. I quote this machine as being a metal covered monoplane, which type our "technical" (and non-technical) Press appear to consider the last word.

This machine weighs, fully loaded, 10,000 lbs., and weighs empty about 6,300 lbs., with three 220 h.p. engines. Using conservative figures, we could, with the best modern British practice, construct a biplane to the same total weight, but of about 900 lbs. lower empty weight: hence it would carry about 900 lbs. more useful load, and this machine would have the same landing speed and the same top speed.

America does appear to be giving us a lead in making use of aeroplanes and in acclaiming their potentialities; she does, I think, show more enterprise and more daring in producing them, but, speaking as impartially as is possible for a foreign rival, I do think we are still competent to design and to construct rather better machines.

EXPERIMENTAL TEST FLYING

BY C. F. UWINS.

Paper read before the Bristol Branch, March 21st, 1929.

It is my privilege to speak to you to-night on some aspect of the work of testing experimental aircraft, and in order to present my subject to you in as lucid a manner as possible, I am going to ask you to consider that we actually have a machine before us waiting for its first test.

We must assume that the cockpit arrangements have been approved before this, for so much has to be done that can only rightly be done as the machine is being built. The seat and rudder positions must be comfortable, windscreening good, view ample, engine controls handy, flying controls accessible, smooth and firm, petrol flow satisfactory and necessary instruments must be clearly visible. The first thing to be done is to weigh and balance the machine. From this we learn the total weight—the designer learns whether he has been clever or not—and then we chase that elusive spot the C.G. The wing position and tail setting have been designed with the C.G. in an assumed position, and if one weighing shows it to be seriously out, then before the machine can be flown it should be ballasted to bring it to the required balance. The flying controls should be carefully tested by the pilot. It is not sufficient to waggle the joystick and watch the ailerons and elevators move; very careful attention must be given to see that they move the right way.

The engine can now be started and things become really interesting. The engine mounting must be carefully watched for vibration, and in fact the whole machine needs carefully watching to determine that all parts are steady with engine running. Throttle gearing must be noticed; it may open much too rapidly and such a fault in service would cause a lot of trouble; but imagine that all is well, we move the chocks away and are out on a taxying test. This serves two purposes; primarily it tests the undercarriage and tail skid, and a reasonably severe bumping and turning is permissible, for remember if there is to be a failure, and that is what we are on the look out for, it is better that it should occur at 20 m.p.h. than when landing at 60 m.p.h. So choose a few rough spots on the aerodrome, harden your heart, forget designers and insurance companies and produce 60 m.p.h. effects at 20 m.p.h. speed. While you are doing this an eye on the centre section cross bracing may prove instructive, for this gets a particularly bad time on rough ground. It may prove to be too slack when the wings will move from side to side over the fuselage, and in one case I remember, cross bracing however tight in the front of the centre bay proved quite inadequate, for the wing tips moved backwards and forwards as well as up and down. Taxying revealed the need for cross bracing in the rear of the centre bay as well as in the front, and flying was postponed until this was fitted. The secondary and no less important gain from the taxying test is that it accustoms the pilot to the feel of his new machine. At first the outlook has been strange, but it rapidly becomes familiar and the hand falls naturally to the throttle. The undercarriage is now inspected, cables tightened, and we are ready to go a step further. Here I have found one or two short straight hops invaluable; each control can be felt in turn, it is a very short hurried feel, but you do know it works and you are becoming still more accustomed to your new position. All having proved to be well, back we go to take the longest run possible, and as the Americans say, "Give her the gun." A short run, for the machine is light, and we are in the air, and away on a straight climb to 2,000

or 3,000 ft. Errors in the fore and aft trim at once become apparent, the machine may want to climb rapidly and must be held down until the range of the adjustable tail plane has been utilised to correct them. All going well we carry on to our desired height where we are safe to try the effect of the controls. Try the ailerons gently first and notice the response of the machine, and then the rudder will help the turn, and you will at least know that they all have some effect for the elevators have proved themselves on the climb. Now throttle back the engine, slowly approach the stall and read the air speed. Do not actually stall, for your controls are not yet tested to stop the spin that may result if you do, but you have discovered your gliding speed, which should not be less than about 20 m.p.h. above the stalling speed of the machine. Try the controls near the stall and note the response. The elevators may be good, the ailerons fair, and the rudder negligible, and it is now our business to so modify them, that the completed machine has no faults or vices.

Here it must be explained that the design of controls varies considerably with various types of machines, and becomes progressively more difficult as the speed range and manœuvrability increases. In a machine of the Moth type, the controls have to function at from 45 to 100 m.p.h., so that a fairly large aileron to give control at 45 m.p.h. does not offer a very big load to the pilot at full speed of 100 m.p.h.; but on a fast single-seater, like the Bristol "Bulldog," the speed range is much greater from 55 to 180 m.p.h., and in addition fighting requires a very high rate of manœuvrability, and it is not sufficient that the aileron control is enough, and the rudder control ample; these controls must require comparable effort to operate them so that the flying of the machine is a harmonious affair. Another point which must be appreciated is that a certain degree of stability is required, but this must not be more than is absolutely essential.

For the information of those who are not quite clear on the subject, a perfectly stable aeroplane is one which will return to its normal flight path after it has been deviated therefrom by some outside disturbance. Longitudinal stability ensures the righting of the machine after the nose has risen or fallen, for the comfort of the pilot is of profound importance; but it must be realised that a large stabilising movement can have its disadvantages. On a large passenger carrying machine, which has to manœuvre slowly, it is excellent, but on a single-seater, or any machine where quick manœuvring is essential, it is constantly fighting the pilot's movement of the elevators, and must therefore be kept at a minimum value commensurate with comfort and safety.[1] So here you see the chief problems facing us in the testing of a new machine.

It has to be stable in all senses, but the degree of stability will vary with various types.

It has to be manœuvrable at all speeds, and particularly so near the stall.

The controls have to be well co-ordinated so as to require the same effort to operate them.

Now where are we to begin? Some controls may appear satisfactory at first, but as tests proceed they may need modification to ensure that they harmonise with the others. A little consideration decides that we begin on the tail controls and preferably the fin and rudder, for the wing would normally fly quite satisfactorily by itself and the designer knows all its characteristics from wind tunnel data. The unknown quantity is the fuselage and the tail surfaces, and the most important and difficult of these in my estimation are the fin and rudder. So let us imagine that all controls are in some way unsatisfactory, and we will tackle the fin and rudder first. Their purpose is two-fold. The rudder controls the direction of the machine. The fin and rudder area combined is responsible for the directional stability and must determine that the centre of pressure on the side of the fuselage is always behind the centre of gravity. For an aeroplane can be compared with a weather vane; the shaft of the weather

vane is the fuselage of the aeroplane, the rear feathered part of the arrow is the rear fin surface, the pivot of the vane has its counterpart in the c.g. of the aeroplane; and in the case of the weather vane it is stationary and the wind is moving, while with the aeroplane it is moving and the air relatively is stationary, which has the same result. You can quite well imagine what would happen if the feathered part fell off the arrow of the weather vane; it would immediately turn round and the arrow head would point down wind and not up wind and if the feathered part were too small the arrow would not be stable and would blow about badly, so that the feathered part must be large enough to ensure that the centre of pressure on the side of the wind vane is always behind the pivot point, when it will always point up wind. And the same applies to an aeroplane. Now to find out if your fin and rudder area is sufficient the following procedure will serve. If you have been flying in bumpy air you will already have some idea if all is as it should be, for if the fin area is at all too small the machine will have been swinging about in an unpleasant manner as our weather vane did just now, but if you are flying in calm air you will not have received any such indications. You therefore produce your side gust by side-slipping at quite a good speed and use your rudder to keep the nose just below the horizon. If you have to use bottom rudder your fin surface is insufficient, while if you use top rudder at least you have enough. If no rudder at all is required the fin area is not quite enough and must be increased. And so you can arrive at an area which will give you directional stability.

The position now becomes more involved; you know that you have enough fin, but you do not know whether you have too much, and too much, while not being dangerous, can make the machine uncomfortable to fly. It really produces a case of directional instability, where we have too much fin for the amount of dihedral. The fin area and dihedral are very closely related, and a good way to proceed with the tests is as follows:—Trim the machine to glide hands off, and then push on rudder on one side and then centralise it. This will cause the machine to bank violently to that side. If the fin and dihedral proportions are correct the machine will slowly right itself and glide on its normal path. If, however, the fin is too big the machine retains the bank and develops a spiral glide which gradually becomes steeper and the pilot is compelled to correct it. If this happens, one of two remedies lies before you, either to reduce the size of the fin or, if that is impossible, to increase the dihedral angle.

Up to this point we have examined the following:—

(1) Lateral and spiral stability.
(2) Total rear fin surface.

The rear fin surface has now to be broken up into its two components, fin and rudder, and the rudder is the most important control with which we have to deal. It must fulfil the following requirements:—

(1) Offer good directional control at all speeds.
(2) Be light to operate.
(3) Be capable of correcting a spin should one develop. This really only applies in the case of aircraft up to about 4,000 lbs. weight.

Investigation of point No. 1 comes first. The machine is stalled carefully and the rudder tried as the stall is approached. A reasonable response is required, and if it is not obtained at first, two modifications offer themselves for consideration—either gearing up the existing rudder, or increasing its area, or both. But modifications along these lines will finally give you the desired result. At the same time tests must be made operating it at the highest speeds of which the aircraft is capable, so as to know that the effort required to operate it is not too great. Should the control be too heavy it will require balancing by one of the normal methods, and care must be taken not to overdo this or yawing will take place over small angles at high speeds.

Consideration No. 3, the control of the machine while spinning, must be left for some time until the other controls have received some attention. I fear I have devoted rather a long time to the rudder and fin, but in my estimation they are the most important of all the control surfaces. What we have done is to get this important control right, and from it we have a useful guide as to the feel and rate of response we must obtain from the other two controls.

The ailerons do not present a very great difficulty; we have the same considerations, control at the stall, and lightness of operation at high speeds, but Frise balance ensures the latter and sufficient area guarantees the former and with a little experiment it is not difficult to produce a control which harmonises well with the rudder.

The tail plane and elevators require rather more consideration. Possibly the best thing is to test first of all the pitching stability of the machine. To do this, adjust the tail plane for longitudinal trim so that the machine flies "hands off." Then disturb this normal flight path by pushing the joystick forward until the speed has increased by 10 m.p.h. Then release the stick, and if the machine is stable the nose will rise above the level position, stop, then fall below the level, only not so far as before, and so in a series of gradually decreasing oscillation will finally return to the level flight position. Should it be unstable it will not attempt to recover from the dive produced by pushing the stick forward in the original instance, but will gradually become even steeper until a vertical dive results. A fighting aeroplane does not want to be too stable in the longitudinal sense, and if it is, a decrease in the area of the tail plane may be advisable. Having decided that the degree of longitudinal stability is satisfactory, the elevators must be examined at the stall and at full speed, and the area, gearing and balance modified to obtain the same degree of response as has already been obtained on the other controls.

On one or two occasions I have referred to the control which regulates the angle of incidence of the tail plane. This is of very great importance, and in the manner of its use the tail plane is a very coarse elevator. It has to be used for two main purposes and they are:—(1) To counteract variations in the angle of down-wash off the wings; (2) to counteract any couples which may be set up or removed when the engine is throttled down. Considering the first, you know that the air leaving the wing is moving downwards and this down-wash varies in angle according to the speed of the machine and wing section used. At high speeds it may be flowing down at several degrees less than at low speeds, but at a much greater speed, so that if the tail plane were fixed the down load on it at high speeds would be too great, and at low speeds it would be insufficient. In the first case the pilot would have to maintain considerable pressure on the elevators to keep the machine on a level keel, and in the second case he would find the machine very nose heavy and difficult to land. The tail plane trimming gear therefore gives the pilot a choice of tail settings, and he is always able to adjust the trim of his machine so as to reduce the load on the control to a minimum. It becomes just as much a part of flying the machine as moving the joystick. The second use is when the line of thrust is above or below the centre of resistance of the aircraft. Throttling back the engines will remove the force of a couple which the tail plane has been set to counteract. If the line of thrust is above the centre of resistance, throttling the engines will produce a tendency to stall, which is only controlled by forward pressure on the elevators until the incidence of the tail plane has been increased.

Now the original setting of the tail plane is a matter for designer of the aircraft, and he has to assume angles of down-wash, the drag of various parts, etc., and so decide what complete range of movement is necessary. And we have to find if his assumptions are correct, and the range of movement is satisfactory for all conditions. The operation of the tail trimming gear must be

possible at all speeds, and it should be positively locked or irreversible. On some machines, with the lever operated type of tail plane mechanism, it is impossible to release it at high speed as the load on the lever is too great. With the wheel operated type this disadvantage is not encountered, although owing to the increased load on the surface, greater effort will always be required to operate any control at high speed.

I think we have now devoted a certain amount of attention to all controls and we must assume that the machine, if of the Scout class, is ready for spinning and diving tests. Prolonged spinning tests are now obligatory on all single-seaters, as in the past one or two have exhibited vices and would not come out of the spin. These tests are not pleasant as the machine spins fast, and after all you are up to discover if it will come out. Get up fairly high and develop the spin progressively. Two turns first, and then stop; if all is well the next can be slightly longer, until finally you can complete the ten complete turns which the Air Ministry consider essential. This must be done both to left and right. If the machine shows a tendency not to recover it may be necessary to increase the rudder size and reduce the fin, and in very obstinate cases a longer fuselage has proved an advantage, as the tail has been blanketed by the close proximity of the wings.

The diving tests consist of a dive at terminal velocity and the whole machine must be steady throughout. It is an excellent test for flutter and, like the spinning tests, must be carried out progressively, the speed being increased by 20 m.p.h. in each dive until the maximum is reached. This test has proved instructive, as when first carried out the fuselage fairings were found to collapse and needed considerable strengthening.

No tests are complete without performance figures and the methods used in obtaining these may be of interest. A complete set of reliable instruments is essential, and in addition to the normal instruments carried on the machine, a strut thermometer has to be carried to record the temperature at all heights. The machine is carefully ballasted to its fully loaded weight and partial climbs are then carried out at about four different heights. In the case of a machine with estimated ceiling of 30,000 ft., these would be done at about 6,000, 12,000, 18,000 and 24,000 ft. The procedure is as follows:—The machine is taken to the desired height, and short climbs of 2,000 ft. made; for example, at 6,000 ft. height the climbs will be from 5,000-7,000 ft. These are conducted at various speeds from just above the stall to just above the maximum speed, when it becomes a descent, and is called a descending partial. These results, when plotted, show the optimum rate of climb at that height and also give the technical office much other valuable data. From these partial climbs you are able to plot your best climbing speeds at all heights and a climb from ground to ceiling is now carried out. On this the pilot takes readings every 2,000 ft. or so of height, time taken, r.p.m., air temperature and other details which are likely to be of interest. Level speeds are taken at ceiling and on the way down at various heights. The figures show whether the propeller is suitable and some improvements in performance can frequently be obtained by modifying the propeller. Engine temperatures also have to be checked very carefully; temperatures of plugs, oil outlet, and crankcase will reveal whether the engine is over-cowled or vice-versa, and if under-cowled some improvement in performance may be obtained by closer cowling.

I have tried to indicate in the foregoing some of the problems a test pilot has to face in the development of new machines. I have had to deal with the perfecting of all controls and this may leave the impression that all controls on new machines are always open to improvements. This is not always true, for aeronautical knowledge has advanced so rapidly that there is a very great difference in testing a machine ten years ago and doing so on one of modern design.

The wind tunnel has taught us a great deal. Directional stability to-day is almost certain on any new machine, whereas previously it was most distinctly a matter for trial. Test flying has its great attractions and occasionally its anxious moments, but it is the branch of aviation in which one appreciates at first hand how much we still have to learn.

I have not found it possible in the time allotted to me to deal with the test pilot's work in the development of new engines. This is a branch in which he can be of particular value, and a great deal of experience is necessary in order to enable him to offer constructive criticism of the faults he is going to discover in the air. These faults are rarely noticeable on the ground, for if they were they would probably have been remedied, but in the struggle for still greater efficiency they are certain to present themselves from time to time. The whole secret of efficient test work must lie in co-operation between the pilot and the designer. The designer must have faith in the pilot's ability to locate faults, the pilot must believe in the designer's ability to produce good aeroplanes and engines. It is a pleasure to be able to say that the disappointments the pilot occasionally suffered years ago in this connection are now becoming more and more a thing of the past.

Discussion

Mr. Pollard said that there had been less progress in the matter of controls compared to that made in other phases of the design of aeroplanes. Could not there be "rules of thumb" for the determination of the size of control surfaces required, the amount of balance necessary, and so forth?

He expressed the opinion that we possessed less knowledge of controls and stability than we had of structural questions; we could guarantee to design new machines that would not break in the air, but could not be assured that controls and stability would be found satisfactory. He asked if it were a fact that about 90 per cent. of the novices time was taken up in learning the art of landing correctly, and in view of the expense of learning to fly, whether the lecturer thought it likely that progress in the direction of the controllability of aeroplanes would make it easier to learn to fly by being easier to learn to land.

Mr. Tinson said the diagrams and experiments which the lecturer had shown brought out the characteristics of aeroplane piloting in a very simple and beautiful manner. Although we were apt to think everything was known about stability and control, the lecture had shown us that there is still so much for which we have to rely on the test pilot.

The principal requirements seemed to be:—

(1) Control down to stalling speed.
(2) Co-ordination of the three controls.
(3) Balance of control surfaces.

He would like to ask the lecturer whether it was not a fact that any machine will remain in a spin if left to spin long enough? Provided that the area of the tail surfaces were fixed in the usual way, he could see no reason why a machine *should* come out of a spin unless made to do so under the influence of the pilot's actions.

With regard to down-wash, had the lecturer experienced a greater difference in trim, engine on and engine off, in the case of machines whose wings made a considerable angle to the propeller shaft? As down-wash was a function of the lift coefficient, it seemed reasonable to suppose that there would be least difference in trim, engine on and engine off, if the propeller shaft were arranged with respect to the wings at the angle of no-lift of the wing section.

As the lecturer had stressed the importance of determining the exact amount of fin surface in proportion to the area of the fin plus rudder, would he enlighten

us as to why certain machines, originally supplied with a fin, had been modified and fitted with a balanced rudder without any fixed fin in front of it? Also, what could the lecturer suggest as a guide to the designer in fixing the gear ratio of controls in the first instance. The question of control gearing seemed wrapped in a certain amount of mystery and apparently answered to no natural laws at all.

Professor PIPPARD asked if the lecturer had any experience of accelerometer tests and of slots. He observed that a difficulty experienced was that pilots and designers spoke different languages—pilots, for example, spoke of centres of pressure, whilst designers talked of Mq's. The position was further complicated by the absence of quantitative reports on degrees of stability.

REPLY TO DISCUSSION

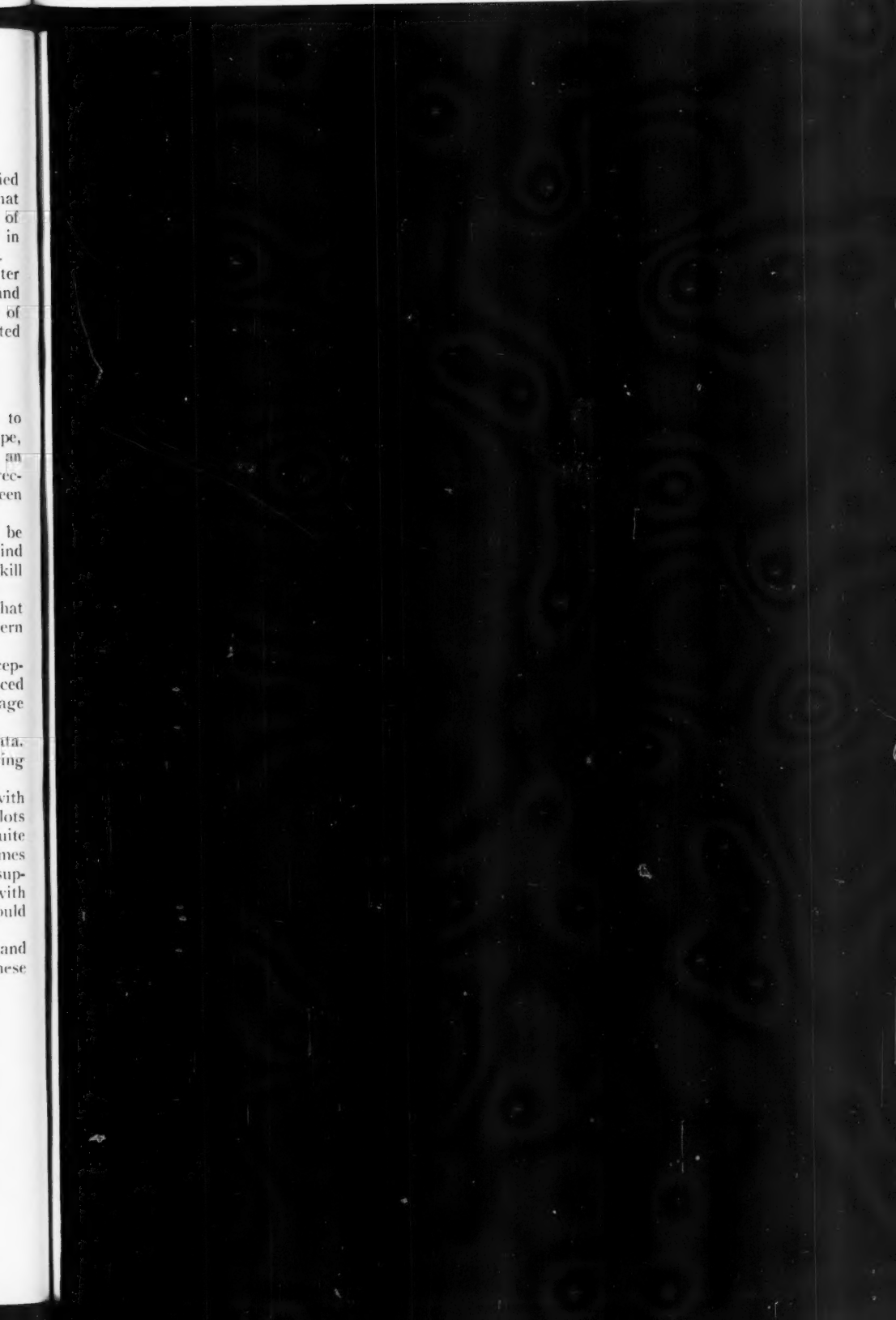
In reply to Mr. Pollard, the lecturer said he did not think it feasible to design controls by rule of thumb, because the influence of the fuselage shape, cockpits, etc., on the flow of air at the tail was impossible to forecast. As an instance, a machine with a Puma engine which was originally unstable directionally, was found to be directionally stable after the engine cowling had been altered to a more streamline shape.

The lecturer said he was of the opinion that skill in landing could not be eliminated. It was necessary to get used to handling an apparatus of any kind that was doing at least 50 miles per hour, but in any case the degree of skill called for was not great and it was largely a matter of practice.

In reply to Mr. Tinson, with regard to spinning indefinitely, he said that some machines will come out of a spin by themselves if left alone, but modern fighting Scouts will not come out unless made to do so by the pilot.

The lecturer had no information as to down-wash on machines with exceptionally large angles between the wings and the propeller shaft. As to balanced rudders without fins, there may be sufficient "fin" surface in the fuselage without any additional fin surface.

With reference to control gearing, it was a question of accumulated data. He had sometimes found that sufficient response could be obtained by altering the gearing, but the response was then abrupt.

In reply to Professor Pippard, the lecturer said he had done very little with accelerometers during his test work, but had some experience with slots. Slots showed up to great advantage on the Bristol Fighter, which had become quite unpleasant when heavily loaded. These machines were now carrying at times something like 1,000 lbs. more load than the first machine of the type was supposed to carry, but even with this over load could be flown at slow speeds with comfort when provided with slots. They could be spun if required, but would not spin themselves.

The lecturer said he was of the opinion that standardisation of control and stability was not feasible. Individual opinions would always vary on these questions.